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CONDITION OF THE MOON'S SURFACE.

By R. A. Proctor B.A., F.R.A.S.

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I. ON THE PROBABILITY OF ERROR IN EXPERIMENTAL RESEARCH.

By William Crookes, F.R.S., &c.

A SCIENTIFIC man engaged in any special pursuit has much difficulty in making clear, to even the scientific public, the result of his experiments. The benefit of his research may be perfectly apparent; but if his experiments have been conducted with rigour there will be a certain individual departure from a general standard in the results, which, if he merely state his conclusions, will confuse the attentive reader. Perhaps certain of the experiments were performed under better test conditions, and their numerical results are therefore more nearly correct than the results of another series of experiments. Should this be the case, to take an average of the results would yield an empirical result, deviating considerably from the truth. Yet many of our most eminent experimentalists are satisfied with recording their experiments, and leave the student of their labours in an uncomfortable uncertainty as to the exact value of the entire system of experiment.

In his "Budget of Paradoxes," Prof. De Morgan, in the consideration of the relation of facts to theory, asks the question—"What are large collections of facts for? To make theories *from*, says Bacon; to try ready-made theories *by*, says the history of discovery; it's all the same, says the idolater; nonsense, say we!" Whether, however, we take facts in subordination to theory, or the reverse, matters little for our present purpose; we have to regard the observation of facts, ascertained experimentally or otherwise, as the test of theory. But a difficulty immediately occurs to the experimentalist, and may be framed in the question—"Which, and how much, of these experimental facts am I to regard as correct, absolutely or approximately?"

Absolute correctness evidently may not be expected of any of the human senses, since the absence of error would

include perception and correct estimation of the most minute deviation from the absolute standard. Between even this deviation and the absolute standard there are an infinite number of results that may be obtained. Approximate correctness is all, then, that the experimentalist has the probability of attaining. So that, given a series of experimental results, there remains the decision of two points—(1), the determination of the standard; and (2), the probable error of the assumed standard.

There are many engaged in experimental research, even in research of a relatively high order, to whom the methods of determining these two points are unknown. To these I shall endeavour to explain the laws of probability as they have been laid down by eminent mathematicians. To those (and I am afraid their number is not legion) acquainted with these laws, I can offer illustrations only of the application of these laws. Without a due consideration of these principles astronomy could not claim its character of exactness; and there appears no reason why the physical and chemical sciences should not, as means of observation increase in delicacy, attain to the rank and estimation of exact sciences. Our chronographs measure easily to the 1-100,000th of a second of time; our balances turn with a fragment of a hair weighing 1-10,000th of a grain; the results of electrical experiments have been obtained varying only 5 in the 1000. With this exactness, surely we may think it carelessness that does not ascertain the closest approximation to accuracy, as well as the limit of error to be allowed this approximation.

The subject is a most subtle one. It may be defined as the best mode of combining observations so as to yield the most trustworthy mean; and in this light I am unable to mention any work affording so popular and so profound a discussion as the little volume, by Prof. De Morgan, entitled "An Essay on Probabilities." The author shows how the observer may *measure* the degree of confidence to which the *average* of any series of observations is entitled. Thus, taking his own example,—that of fifteen observations giving the following results: 722, 933, 1033, 917, 1311, 1089, 972, 1294, 967, 1344, 1250, 744, 1309, 858, 1029,—if the average, or the arithmetic mean, of all the observations be 1051, the theory does not assist the observer in estimating the probability of this average being *true*. For *true*, in the absolute sense of the word, it, in all probability, is not, and therefore no theory is needed to assist in drawing that conclusion. But let any definite departure from truth be

named, then the theory enables the observer to determine the degree of likelihood that his average is within such limit. Thus it is found that the probability of the average of 1051 being within 50 of the truth (whatever the truth may really be) is 0·66; and therefore the observer should contemplate the possibility of his average being within 50 of the truth, or, which is the same thing, that the truth lies somewhere between the limits 1051 ± 50. This he would assume with the same degree of confidence, neither more nor less, that he would yield to a witness who is known to speak truth 66 times and falsehood 34 times out of every hundred.

If the calculation had been made for the limits 1051 ± 10, the resulting probability would have been much less than 0·66; and if for 1051 ± 100, the resulting probability would have been much greater. This is in accordance with common sense.

It must also be very important to the observer, when he has made different *sets* of observations, to know how best to combine their respective averages. For both purposes the average of the set, or of each, may be "weighted" by means of the formula—

$$W = \frac{n^2}{2\Sigma e^2},$$

where n = the number of observations and Σe^2 = the sum of the squares of the successive differences obtained by subtracting each observation from the arithmetic mean (average) of the whole.

The term *weight* is almost self-explanatory. Of a series of observations there may be one which the observer considers to have been obtained under more favourable conditions than the others, and to which, in the balance of judgment, he should accord greater "weight." For if we suppose a satisfactory experiment to give 5, and one of unequal weight 6, it would be obviously unfair to take the average as $5\frac{1}{2}$; but it would be more reasonable to give the result 5 the advantage of supposing it to have occurred, say, three times to the occurrence of the result of 6 once. This would be giving the observations 5 and 6 the weights of 3 and 1, and the average would be $5\frac{1}{4}$. Such a mode of reasoning gave rise, before mathematicians had constructed the theory of probabilities, to a rule for finding the average, which may be quoted as follows:—Weigh every observation, multiply it by its weight, take the sum of the products, and divide this sum by the sum of the weights. But it was found that the

theory of probabilities dictated a variation of this rule, embodied in the formula previously stated. This formula may be understood in words as follows:—Square the number of observations, and divide this product by twice the sum of the squares of the errors.

Having now defined the term "weight," we have to trace the meaning of the terms *mean risk* and *probable error.*

If we consider positive and negative errors as equally probable, they will balance each other, so that the average of positive or of negative errors will be equal. We thus arrive at the meaning of the term *average error*, and can proceed easily to the determination of the *mean risk;* and as the mean risk of positive error is the average positive error, the mean risk of negative error the average negative error, the mean risk may be taken as half the average error. Representing the mean risk by m, and the weight by w, we have—

$$\text{Mean risk} = \frac{200}{709\sqrt{w}},$$

$$\text{or more nearly,} = \frac{0{\cdot}2820953}{\sqrt{w}}.$$

$$\text{Or, mean risk} = \text{prob. error} \times 0{\cdot}591473.$$

The *probable error* is that error for which there are equal chances of exceeding or of not attaining. For instance, suppose the chances are equal that the error is included between 0 and 2, or that it should exceed 2, assigning this as the limit of error, then, of course, for any number greater than (say) 10 the chances are in favour of the error being included within the number. It has been calculated that—

$$\text{The probable error} = \frac{62}{130\sqrt{w}},$$

$$\text{or more nearly,} = \frac{0{\cdot}476936}{\sqrt{w}}.$$

$$\text{Or, the probable error} = m\,1{\cdot}690694.$$

Calling the mean risk m, the probable error p, and the weight w, we have from the preceding reasoning the following formulæ:—

$$w = \frac{113}{1420m^2}.$$

$$w = \frac{5}{22p^2} = \frac{0{\cdot}227468}{p^2}.$$

From the foregoing formulæ, when either the weight, probable error, or mean risk is given, the other two can be determined. As we are capable, in most scientific observations, of so adjusting our instrumental means that the errors

may have positive and negative signs, so are we now in a position to verify these observations, or select those most closely approximate to the truth.

But there have been tabulated the values of the celebrated definite integral—

$$\mathrm{H}_t = \frac{2}{\sqrt{\pi}} . \int_0^t e^{-t^2} . \, dx \, ;$$

and from these tabulated values (for the extension of which we are again indebted to Prof. De Morgan) we may, with much less trouble and more accuracy, arrive at the desired result. The values have been arranged in two tables: the columns of Table I. are headed thus:—

t.	H.	Δ.	Δ².
0·50	0·52049 99	874 38	8 88
2·17	0·99785 11	9 96	44
2·68	0·99984 94	84	5

t represents every hundredth of a unit from 0 to 2.

H represents the values of the area enclosed by an asymptote, this asymptote continually approaching but never reaching the abscissa, the whole of the enclosed area forming one square unit.

Δ represents the differences of these values.

Δ² represents the differences of these differences.

In Table II. are three columns only, *t*, Δ, and K,—a modification of H,—headed thus:—

t.	K.	Δ.
4·6	0·99808	40

Let us now take an illustration. There are ten observations of which the arithmetic mean is—

$$a = 200{\cdot}01577.$$

The sum of the squares of the ten differences between a and each individual observation is—

$$\Sigma e^2 = 0{\cdot}00000007.$$

Hence the weight of a is—

$$w = \frac{\text{(number of observations)}}{2\Sigma e^2} = \frac{100}{0{\cdot}00000014} = 715000000.$$

The largeness of this figure indicates the high degree of probability that a is very near to the true value sought. But the query may be put—What is *the* true value? It will be seen that the question does not admit of absolute answer, and for the following reasons:—One of the observations

was (say) as low as 200·0156; one was as high as (say) 200·0159. Further observations might have yielded results still more extreme. But assuming they would not, still the number of possible values between 200·0156 and 200·0159 is infinite. The arithmetic mean a is but one of these, and although more likely than any other that could be named, it is not more likely than one or other of all the possible values. The odds are more than 1000 to 1 that a is not the truth; but they are also more than 1000 to 1 that a is very near the truth. The question—How near?—cannot be answered. Alter the question to—What is the probability that the truth is comprised within the limits $a \pm k$?—and the answer may easily be given, however small k may be. Thus, if $k = 0{\cdot}0001$. In other words, if the question be—What is the degree of likelihood that the truth lies between 200·0157 and 200·0159?—the answer is given by the formulæ—

$$\pi = \mathrm{H}_t,$$
$$t = k\sqrt{w},$$

where k, 0·0001,
„ $w = 715000000$, and as log. $w = 8{\cdot}854$, log. $\sqrt{w} = 4{\cdot}427$,
„ $\sqrt{w} = 26800$.
Then $t = k\sqrt{w} = 2{\cdot}68$, and from Table I.—

$$\pi = \mathrm{H}_{2\cdot68} = 0{\cdot}99985.$$

The result, 0·99985, is so near to unity (the measure of certainty) that for every practical purpose it may be considered certain that the truth is really comprehended within the limits named.

Take six other observations, the arithmetic mean of which is—

$$a = 203{\cdot}870\,;$$

and the sums of the squares of the six differences between a and each separate observation is—

$$\Sigma e^2 = 0{\cdot}71515.$$

Then the weight of a is—

$$\frac{36}{2\Sigma e^2} = \frac{18}{\Sigma e^2} = \frac{18}{0{\cdot}71515} = 25{\cdot}2.$$

The smallness of this figure, contrasted with the 715000000 of the preceding example, indicates a comparatively low degree of probability that the true number is comprised within the limits $\pm k$, when k is very small. For instance, suppose the question to be—What is the likelihood that the true number is comprised between 203·77 and 203·97? As

before, the probability will be determined by the given formula.

If $k = 0{\cdot}100$,
$w = 25{\cdot}200$,
$\sqrt{w} = 5{\cdot}020$,
$t = 0{\cdot}502$,
$\pi = H_{0{\cdot}502} = 0{\cdot}52$.

This result, 0·52, is just intermediate between unity or certainty and zero, the lowest degree of probability, otherwise denominated impossibility. Hence, that the true number is within the limits stated is just as likely as the throwing of Head with a single halfpenny, but not more likely.

I will now submit a practical application to the reader.

The value of the results obtained during any series of experiments must of course vary with the care taken in the performance of the individual experiments. In support of this view I have, in the practical application of the laws I here endeavour to simplify, taken the utmost pains to ensure accuracy. The application is the determination of the atomic weight of thallium; and I shall first enumerate the means (not usually employed) by which I deem accuracy to have been ensured, and then proceed to evolve the results.

With a metal of so high an atomic weight (203·642) as thallium, errors and inaccuracies comparatively trivial with elements of low atomic weight, are magnified into alarming proportions. Impurity of the reagents employed, imperfect manipulation, but, more than any, the inaccuracies arising during the weighing from the omission of the corrections required by temperature, pressure, &c.,—all these influences must be eliminated in the determination of an atomic weight.

The atomic weight was derived by two methods:—First, by taking a known quantity of metallic thallium, dissolving it in nitric acid, and weighing the nitrate of thallium produced. Secondly, in dissolving known quantities of sulphate of thallium in water, and ascertaining how much nitrate of barium is necessary to precipitate the sulphuric acid as sulphate of barium.

There were also two methods of weighing: one in air, at ordinary pressure and temperature, and one in a highly rarefied atmosphere. For the first method a balance was employed, made expressly for the work by Messrs. Keissler and Neu, which will indicate clearly a difference of 0·0001 of a grain when loaded with 1000 grains in each pan. For the second method of weighing a balance was employed

which I term a *vacuum* balance. It was made by Oertling, and is a duplicate of the first, but it is enclosed in a cast-iron case, connected with an air-pump, and arranged for the weighing to take place in air of any desired density. The best manner in which to use such a balance as this is to introduce a certain approximate weight, and then to alter the pressure of the air until the balance shows equilibrium. Two weighings, at different degrees of atmospheric pressure, varying by a considerable interval, give data upon which to calculate what the weight would be in a perfect vacuum. For the full elucidation of the formulæ employed, for the method of adjusting the standard grain-weights according to their value *in vacuo*, and for the preparation of the glass apparatus and the pure reagents, I must refer the reader to my paper *in extenso*, contributed to the Royal Society.* But I may, from the abstract of that paper, collect the results of a series of the weighings. They were as follows:—

The weight of the glass + thallium.
The weight of the glass + nitrate of thallium.
The weight of the glass alone.

	Grs.
True weight of thallium *in vacuo* .	=183·790232
True weight of nitrate of thallium *in vacuo*	=239·646066
True weight of glass	=766·133831
(*a*) Weight of thallium according to true value of weights in air	=183·783921
(*b*) Weight of nitrate of thallium in air (1005·425937—765·814578)	=239·611359
(*c*) Weight of glass, &c., in air .	=765·814578
Weights employed to balance (*a*) .	=183·8099
Weights employed to balance (*b*) (1005·4364—765·8081) . .	=239·6283
Weights employed to balance (*c*) .	=765·8081

From these data the atomic weight can be deduced by simple proportion, but the results of the statements of the proportion are absolute only if the atomic weights of nitrogen and oxygen are correct. The determinations of Prof. Stas show that the atomic weights of nitrogen and oxygen should be represented by N = 14·009, and Θ_3 = 47·880, instead of N = 14 and O = 16, as hitherto more generally held. The equivalent of nitric acid thus becomes $N\Theta_3$ = 61·889, instead of the old equivalent NO_6 = 62. Taking as data Prof. Stas's determination of the atomic weights of nitrogen

* June, 1872.

and oxygen, and the weights *in vacuo*, the quantity of nitric acid required to convert the thallium into nitrate is—

$$(239{\cdot}646066 - 183{\cdot}790232 =)55{\cdot}855834 \text{ grs.}$$

We have then the proportion—

Weight of Nitric Acid.		Weight of Thallium.		Atomic Weight of Nitric Acid.		Atomic Weight of Thallium.
55·855834	:	183·790232	: :	61·889	:	x;

$$\therefore x = 203{\cdot}642.$$

Substituting the old equivalents we obtain—

55·855834	:	183·790232	: :	62	:	204·007

as the atomic weight; but I cannot admit this number to be so nearly correct as 203·642.

If we take the corrected weighings in air of ordinary density, we have, with $N\Theta_3 = 61{\cdot}889$,

$$203{\cdot}738.$$

With $NO_6 = 62$,

$$204{\cdot}103.$$

Accepting the uncorrected weights, observed in air, we have, with $N\Theta_3 = 61{\cdot}889$,

$$203{\cdot}162.$$

With $NO_6 = 62$,

$$204{\cdot}165.$$

The error of the last deduction is $+0{\cdot}523$, a sufficiently large number to show the inutility of the application of the theory of probability until every care has been taken to eliminate the errors arising from inaccuracies. As I have stated, in the paper to which I have referred, the largeness of these errors has an immediate bearing upon quantitative analysis, for it is shown that, from data ordinarily given, very varying results may be obtained. Chemists have to deal with much smaller quantities than a quarter per cent, particularly in organic analysis, where so wide a difference from the truth may lead to very erroneous reasoning.

Pass we now to the application of the theory of probabilities to ten results of the most trustworthy weighings. These, with $N\Theta_3 = 61{\cdot}889$, are as in Table I.—

Tabulating the results of the determinations, with the view to ascertain severally their degree of approximation to the arithmetic mean (Table II.)—

The arithmetic mean of the ten observations is—

$$a = \frac{2036{\cdot}424}{10} = 203{\cdot}642.$$

TABLE I.

True Weights *in vacuo*.

Determination.	Weight of Thallium taken.	Weight of Nitrate of Thallium + Glass.	Weight of Glass.	Calculated Atomic Weight from these Data.
	Grs.	Grs.	Grs.	Grs.
A.	497·972995	1121·851852	472·557319	203·666
B.	293·193507	1111·387014	729·082713	203·628
C.	288·562777	971·214142	594·949719	203·632
D.	324·963740	1142·569408	718·849078	203·649
*E.	183·790232	1005·366796	766·133831	203·642
F.	190·842532	997·334615	748·491271	203·636
G.	195·544324	1022·176679	767·203451	203·639
H.	201·856345	1013·480135	750·332401	203·650
I.	295·683523	1153·947672	768·403621	203·644
K.	299·203036	1159·870052	769·734201	203·638

TABLE II.

A.	203·666	+0·024
B.	203·628	−0·014
C.	203·632	−0·010
D.	203·649	+0·007
E.	203·642	+0·000
F.	203·636	−0·006
G.	203·639	−0·003
H.	203·650	+0·008
I.	203·644	+0·002
K.	203·638	−0·004

The sum of the squares of the differences is—

0·000576
0·000064
0·000049
0·000004
0·000000
0·000009
0·000016
0·000036
0·000100
0·000196

$$\Sigma e^2 = 0·001050$$

Therefore $2\Sigma e^2 = 0·0021$; and the weight (w) of a is—

$$= \frac{100}{0·0021} = 47619.$$

* Fully illustrated in the Paper.

We have then, from the formula,—

$$\text{The probable error} = \frac{62}{130\sqrt{w}},$$

$$\frac{62}{130 \times 218} = \log.\ 62 - (\log.\ 130 + \log.\ 218) = \log.\ 0\cdot0022,$$

the number 0·0022 as the probable error. Or by means of the tables calculated from the definite integral we can arrive at a similar result. Thus—"What is the probability that the truth is comprised within the limits $a \pm k$?" If $k = 0\cdot01$; and $\pi = \text{H}$

$$t = k\sqrt{w};$$

$$\begin{aligned} \text{then}\quad w &= 47619, \\ \sqrt{w} &= 218, \\ t = k\sqrt{w} &= 2\cdot18, \text{ and} \\ \pi = \text{H}_{2\cdot18} &= 0\cdot99795, \end{aligned}$$

so near to unity, the measure of certainty, that the number 203·642 may, for all practical purposes, be regarded as the absolute truth. From the second table we can also obtain like results by entering with t. We obtain the argument from the formula—

$$\frac{k}{\text{probable error}} = t.$$

Therefore $\dfrac{0\cdot01}{0\cdot0022} = 4\cdot6 = t$, to which corresponds $k = 0\cdot99808$. There can remain no reasonable doubt, then, that the atomic weight of thallium is $= 203\cdot642$.

As simply as I am able, I have endeavoured to show the application of the theory of probabilities to the judgment of error, and the evaluation of the amount of accuracy in experimental research. The subject has, I think, been involved with undue difficulty. Perhaps it has hitherto been generally held that the results of experimental research have not been sufficiently accurate to permit the refinement; but I must express an opinion quite opposed. Yet I would suggest that, in all kinds of delicate weighings, the effects of temperature and pressure of the atmosphere be taken into consideration. Let me make my meaning clear by an example. There are given to be weighed, let us say, 800 grains of water in 200 grains of glass. First arises the question, —Shall we employ brass or platinum weights for our determination? We shall presently see the difference that would result, in the determination of the absolute weight of the glass and water, from the result of our choice. A brass

weight of 1000 grains will displace 0·1462 grain of air; an equivalent platinum weight 0·058271 grain of air. The 1000 grains of glass and water displace 1·9736 grains of air, so that their absolute weight is 1001·9736 grains. Now the glass and water balanced by the brass weight would give, less the air displaced by the weight, 1001·8274 grains as the true value of the water and glass; while 1001·9736 grains, less 0·058271 grain, give 1001·915329 grains as the value to be ascertained. So, supposing the barometrical pressure to remain constantly at 760 m.m., we have an error of 1·8276 grains per 1000 in weighing with brass weights uncorrected in air, and 1·915329 grains per 1000 with platinum weights at the same barometric pressure. But we know that the barometer does not always record the same pressure. What, then, will be the result of its variation?—the variation, of course, of the weight of air displaced. Now a litre of dry air (at Greenwich), at 760 m.m. pressure and 0° C., weighs 1·293561 grms., and its weight will be proportionately lower at lower pressures. At 740 m.m. the weight of air displaced by water and apparatus will be 1·9216 grains, and at 715 m.m. 1·8890 grains. The weight of air displaced by the brass and by the platinum weight also decreases proportionately. So that, weighing with the brass weight, we have, at 740 m.m., an error of 1·7792 grains on the 1000, and at 715 m.m. an error of 1·7505 grains. With platinum weights we have, at 740 m.m., 1·864863 grains error, and 1·834334 grains at 715 m.m. These discrepancies are too important to be disregarded. For suppose our weighings to have taken place on different days, at different pressures which were not noted, we should have serious error; and the error would be increased with a specifically lighter fluid than water.

Chemists are aware how greatly an error of similar character would influence the determination of the amount of carbonic acid and of water yielded by an organic body under combustion. Suppose the potash bulbs employed in the analysis to weigh 600 grains, there would be displaced 0·366 of a grain of air at 760 m.m. pressure, 0·327 grain at 740 m.m., and 0·316 grain at 715 m.m. Thus if weighings were made at 715 m.m. and at 760 m.m., there would be an increase of weight of 0·02 grain; and this, if 3·5 grains of the organic compound were under analysis, would give an error of 0·6 per cent. Similarly with a chloride of calcium tube, weighing, with its contents, 350 grains, there would be an error which—with the error in the estimation of the carbonic acid—would give a total error of

nearly 1 per cent. Of the effect and importance of such an error it is unnecessary to speak; to all in the least acquainted with analytical research there will appear full reason for the more careful study of the subject.*

These facts clearly show the necessity,—first, of great care and great delicacy in all manipulation connected with experimental research; secondly, of carefully "weighing" the individual merit of each result, and its relative merit in the series of results. How this may be effected I have endeavoured to explain; and I think that there would be no series of observations (to which this or an analogous method has not been applied) but would benefit by the application. The application should of course proceed from the experimentalist himself, but there are many series of results, the members of which have been obtained by different processes, that would be rendered still more practically useful by an evaluation according to some one of the principles of the theory of probabilities. Perhaps in future years the theory may be universally understood, and it will not be required to revert to the elements of the Science.

II. GOLD-MINES AND MILLING OF GILPIN COUNTY, COLORADO, UNITED STATES.

By JAMES DOUGLAS, Quebec.

FOURTEEN years ago a party of miners detected gold in Dry Creek and other spots near the present town of Denver. The news spread; a rush ensued, and exploration was rapidly carried from the plains up the gorges of the Rocky Mountains. Before 1859 had closed, the gulches round Central City, 40 miles distant from Denver, were swarming with gold-diggers; and mining had also commenced on the rich surface quartz of the lodes, whose disintegrated *débris* had supplied the gold that enriched the neighbouring valleys.

In what is now Gilpin County, and within an area whose centre is Central City, and radius about $1\frac{1}{2}$ miles, was discovered, before 1863, a gold-bearing lode at almost every hundred feet; and many of these lodes were yielding gold

* In the course of my experiments with the delicate apparatus employed in this research, I have noticed some curious effects of the action of heat upon gravitating bodies. Led to pursue the investigation with specially constructed apparatus, in air and *in vacuo*, I hope, at no distant date, to bring forward some results.

and matter for exaggeration so abundantly that American brokers were enabled to form, in the cities of the east, no less than 186 public gold-mining companies. The companies generally possessed capital enough to build a mill, but before the mill was running it in many cases happened that the surface rock, which yielded its gold to mercury, was exhausted, and after a few experiments the mill was stopped; and mill and mine have remained closed ever since. A few mines, however, rich enough to bear the loss of from three-fourths to two-fifths of their produce in the mill, have remained open, to testify to the extraordinary richness of the district. As the mills existed they have continued to be used, despite the defects of their work; but unless some better system be introduced mining must languish, for no mines can long sustain such waste.

The present article is a contribution towards the solution of the question, which, as it involves the saving or loss of several million dollars' worth annually of gold, silver, and copper, is well worthy the attention of metallurgists. So abundant is the ore that were mining conducted systematically, and the product of the mines utilised, Gilpin County would probably yield more value in mineral than any district of equal size in the world.

The country rock is granitic, with some gneissic varieties. The lodes have a general E. and W. course, and dip almost vertically. They are very free from faults, and many of them can be traced, running with remarkable regularity, for long distances; but the productive portion rarely exceeds 4000 feet. The deepest shaft in any of them is only 700 feet, and there are few others deeper than 500 feet: it is therefore impossible to predict what their character will continue to be, and whether the gold yield will be permanent; and the changes which have taken place in certain of the lodes, at different depths, are too inconsistent with one another to allow of any deductions being drawn from them. The structure of the lodes is very characteristic of fissure-veins. The walls are usually distinct, and marked often with well-polished schlicken sides. A clay sewage, then a band of almost pure iron and copper pyrites, intermixed with small quantities of blende and galena, or of blende and galena alone, or of all these sulphurets mixed in almost equal proportions, occurs on one or both sides, while the centre of the lode is composed—where the lode is rich—of a gangue of decomposed quartz or felspar, carrying more or less of the same sulphurets. The solid sulphurets of iron and copper, known as No. I., or smelting ore, usually yield

to the miner from 60 to 80 dollars a ton. The copper pyrites carries most gold, and the fine-grained iron pyrites more than the coarse, distinctly cubical, variety. The blende is also associated with gold, and in some mines is the principal vehicle of it, and the galena is invariably argentiferous. This rich ore is always sold to the smelter, as it refuses to give even as large a percentage of its gold to mercury as the less concentrated ores of the body of the lode, where the gold seems to be in a freer form. The second class ore, in first class mines, will usually carry—

1·4 ozs. of gold,
5·6 ozs. of silver,
2·8 per cent of copper.

It is always treated in stamp-mills where battery amalgamation is employed, and not over 33 per cent of the above-named valuable constituents of the ore recovered.*

The proportion of No. I. ore to No. II. ore rarely exceeds one-tenth, and in most mines the quantity is too small to make it worth while effecting any separation.

The width of the lodes runs from 18 inches to 10 to 12 feet. An average width of the really productive lodes may be set down at 3 feet, but they are all subject to contractions and expansions, sometimes pinching to a mere thread, at other times bulging into enormous bunches. Nor are any of the lodes consistently productive. The mineralogical portions are said to run in chimneys, which are interrupted by streaks of poor or altogether barren rock. The term "chimney" has been borrowed from California, but is not applicable in Colorado, as the rich ground does not form continuous vertical streaks, alternating with vertical streaks of barren rock, but irregular regions of rich ore, merging vertically and horizontally into poorer ground. The term "cap" is applied indiscriminately to merely lean and altogether barren ground. Of the latter there is comparatively little; and as the former includes all ore that will not yield 20 dollars of gold to the ton, much that is now left standing in the mines, it is to be hoped, will some day or other be removed with advantage.

Unfortunately the mining in Gilpin County has been as faulty as the milling, owing chiefly to two causes:—

I. The subdivision of the lodes into very small claims.

II. The failure of the companies very generally to work their claims,—which has led to the mines being either let or

* Mr. ALBERT REICHENECKER, in the Berg-Hüttenmännische Zeitung, reproduced in RAYMOND's Report on Mines and Mining for 1870, p. 360.

worked on tribute. In either case the miner, having no interest in the property, aims only at extracting as much ore as he can during the term of his lease, without regard to the future of the mine.

I. To what a degree the subdivision of the lodes has been carried may be judged from the following enumeration of the claims on some of the principal lodes in the district. The list is taken from Mr. G. W. Baker's pamphlet on the treatment of gold ores in Gilpin County, Colorado.

On the Gregory Lode—

	Feet.
The Black-Hawk Co. owns	500
,, Consolidated Gregory Co. owns	500
,, Marragansett Co. owns	400
,, Rocky Mountain Co. owns	200
,, Benton Co. owns	600
,, Russell (Extension) Co. owns	300
,, Briggs Co. owns	250
,, Smith and Parmlee Co. owns	1100
,, New York (Extension) Co. owns	250
,, United States Co. owns	250
,, Manhattan	400
	4750

On the Bobtail Lode—

	Feet.
The Bobtail Co. owns	$433\frac{1}{3}$
,, Stirling Co. owns	$60\frac{1}{2}$
,, Brastow Co. owns	$62\frac{3}{4}$
,, Sensenderfer Co. owns	128
Private owners in small claims own from	700 to 800
	1483

On the Bates Lode—

	Feet.
The Rocky Mountain Co. owns	250
,, Bates and Baxter's Co. owns	300
,, Union Co. owns	200
,, Loker Co. owns	400
,, Gregory Co. owns	100
Private persons	300
	1550

These three lodes have been the most productive in the district, and the most diligently worked. The Bobtail has,

it is estimated, yielded about 3,000,000 dollars' worth of bullion,—no insignificant yield, considering how short is the really metalliferous portion of the lode and shallow the shafts, few exceeding 400 feet. These lodes carry less galena and blende than most others, and a larger percentage of copper. These three lodes run almost parallel, and so close together that the slight convergence in their course westward has given rise to the conjecture that they unite to form the Mammoth Lode, which can be traced for about 3000 feet from a point a little west of the known westernly limits of the Bobtail. This lode is likewise divided into a number of small claims, the longest of those owned by companies being 400 feet. The lode is wide, and the ore highly charged with iron pyrites; strange to say, almost free of gold. But proceeding further west, and crossing a ravine known as Spring Gulch, we reach a group of parallel lodes so similar in course and dip to the Gregory, Bobtail, and Bates, that, though undetected in Spring Gulch, one cannot but look upon them as a continuation of those three lodes, or, if they are really united in the Mammoth, of this lode again split up into several branches. The most notable of this group is the Burroughs Lode, on Quartz Hill, on which—

	Feet.
The Ophir Co. owns	462
,, Gilpin Co. owns	262½
,, Colorado Co. owns	200
,, Burroughs Co. owns	200
,, Cooper Co. owns	50
,, Hardeslis Co. owns	200
,, Pacific National Co. owns	550
,, First National Co. owns	600
,, Gold Hill Co. owns	70
,, Quartz Hill Co. owns	30

This group and the lodes of the neighbouring Nevada district are, as a rule, poorer in gold and copper, but richer in argentiferous galena, than the preceding.*

The ill effects of such a subdivision it is not difficult to conceive. As every proprietor sinks one or more shafts, a vast amount of unnecessarily expensive work is done. Moreover, the chances of individual failure are greatly increased; for unless the owner be fortunate enough to hit a rich chimney of ore, which sinks vertically without inter-

* For a full and accurate description of the most important mines consult vol. iii. of the United States Geological Exploration of the 40th Parallel, On Mining Industry, by JAMES D. HAGUE.

ruption, when he runs out of good ground he is sure to be in unproductive ground from end to end of his claim, and therefore as sure to fail financially. The evil is now, however, curing itself. As the mines have been sunk the water has become more and more troublesome, and combination has been forced upon the owners by the refusal of some to pay their share of the expense of pumping. A process of what is termed "freezing out" has been going on for some time on the principal lodes, which, by a method hardly justifiable, is likely to lead to the desired union of interests, though at the expense of the shareholders of the companies. A mine fills with water; all returns cease; the company's affairs are liquidated, at the suit of the superintendent or some privileged creditor, for perhaps a trifling sum. The property is sold by the sheriff, before perhaps any of the shareholders in the East are aware, and the mine passes into the hands of a few men, who, if they do not acquire the adjacent claims by the same process, will work in harmony with those who do. The temporary suspension of many of the richest mines, and the consequent decrease in production of the district, is, in a measure, due to the systematic carrying out of such schemes. Some small-claim owners are, however, so fortunate that their success makes it difficult to persuade others of the evil of the subdivision system. There is an owner of some 30 feet on the Bobtail who steadily refuses to join a combination, and who cannot be either bought or sold out. He is down some 500 feet, and throughout that whole depth he has been in good pay-ground. He works for a few months, till he has taken out what gold he requires, and then *knocks off* till he needs to make another draft. As he says his gold is safer there than in any bank, he refuses either to sell or exhaust his mine. It is said that during the last spell of 9 months' work he extracted 500 lbs. weight of retort gold, value about 100,000 dols.

II. The second evil, *viz.*, the failure of the companies to work their own claims, is even more detrimental to the future prosperity of the mines than that last discussed. As a rule the affairs of the companies have been grossly mismanaged. Having spent their slender capital, their superintendents have found it more conducive to their ease to let the mines on tribute or on lease than to work them. The mines are sure to yield enough to pay their salaries. The lessees work, of course, for immediate returns; hence there are few mines in Gilpin County which—through this vicious practice of "gouging," as it is termed—have not been riddled in a shocking manner. To save timber the old road-ways

have been removed; the stopes, if filled with poor ground, are blocked for hundreds of feet, so that it is generally impossible, without great cost, to examine the ground left standing, or, if the stopes be empty, they are vast caverns, with the roof so feebly supported by a few slender props that it is with greatest risk one enters them. These defects of the past—due, in chief measure, to the faults of the superintendents, though in part to the ignorance of the miner, who went to his task from a farm in the East or cattle-grazing on the plains without any previous knowledge, far less education—will, it is to be hoped, not disfigure future operations. When better methods of treating the ore are introduced, the miner will wish to reach the once unremunerative but now valuable ground left standing, and the difficulty and expense of doing so will teach him that it would have been cheaper to have properly opened and kept open his mine from the first.

METHODS OF TREATMENT.

Battery Amalgamation.—At the outset of mining, 12 and 14 years ago, when the rich surface quartz carried free gold abundantly, stamp-batteries, supplied with riffles and such appliances for catching the free gold, were employed. When the sulphurets were reached these failed altogether to secure the precious metal, and amalgamated copper plates supplanted the riffles. But it was some time before the mill-men understood the necessity of thoroughly cleansing and amalgamating the plates. To arrest the sulphurets blankets are, in some mills, placed below the amalgamated plates, and the blanketings ground in pans, the mercury in the concentrate sufficing for the amalgamation of the small quantity of gold thus saved. The tailings are frequently further concentrated in tins—those called "hand-buddles." Round buddles have been tried, but found too slow, and to require more attention than the rough impatient workman will bestow.

The stamps are run slowly, never exceeding 30 strokes,—a higher speed interfering with the battery amalgamation, by discharging on the plates too great a volume of water and slime. Amalgamated copper plates are fixed within the battery, under the charging and discharging openings, and form an apron in front of the discharge 10 feet to 12 feet long, and set at an angle of 10° to 14°. Mercury is added every two hours, through the charging-slit, in quantities to suit the richness of the ore: three times as much is introduced as is afterwards recovered.

The gold caught on the plates is, under the most favourable circumstances, only 40 per cent of the assay value of the ore. The quantity of silver saved is inconsiderable. The gold from the blankets, and that in the buddle concentrate, does not amount to more than 5 per cent more; so that, when treating even the most tractable of these sulphurets, battery amalgamation and tailing concentration do not secure more than 45 per cent of the gold, and therefore involve the loss of 55 per cent of the gold, and of all the silver, copper, and lead. As already stated, it is the second class ore, or that from which has been separated by hand the solid sulphurets, and from which has been thrown away stuff too poor for treatment, that is milled.

The benefit of tailing concentration is so insignificant from the simple fact that it is so carelessly and rapidly conducted, that only the very largest and heaviest particles can settle in the voluminous and swift stream of water used. Most of the tailings carry more than 1 oz. of gold to the ton, about 2 per cent of copper, and 15 per cent of iron pyrites and blende galena. The concentrate will consist of almost pure iron pyrites, very little—if any—more copper than the crude tailings contained, and seldom as much as 2 ozs. of gold. Mr. Baker gives the average contents in gold of 45 samples of tailings, from assays made by reliable assayers, Messrs. Schulz and Burlingame, at 27·86 dols. per ton,—the highest assay being 50·40 dols., the lowest 2·21 dols.; 38 samples of dressed tailings contained on an average, according to the same authorities, 42·90 dols. The heavy iron pyrites is increased four to five times by the concentration as effected now; the lighter copper pyrites, carrying the gold, is washed away into the stream. The first act of reform should doubtless be—dress the tailings from the present mills on the same system that slimes are dressed the world over.

In Gilpin County there are scattered over the hill-sides, at the mines, or in the river valleys, where water runs, but where—through perverse mismanagement—steam is nevertheless often employed as the motive power, about 70 mills, with 1300 stamps. Of these many have been idle ever since they were built, and at the best of times not more than half the number of stamps have been in operation. At the present moment, owing to the special but evanescent causes of depression already explained, there are not 300 stamps running. But in 1868-9, when Gilpin County produced 1,267,900 dols. in gold, and in 1869-70, 1,378,100 dols. in gold, the average number of stamps running throughout the

year was about 400,* crushing about 100,000 tons, whose average yield per ton must have been between 10 and 12 dols., for part of the annual production came from the No. 1 ore, smelted by Prof. Hill. The absolute contents of these ores was probably 35 dols. per ton of gold and silver, 3 per cent of copper, worth say 1·50 dols. per unit, and the same percentage of lead, worth say 50 cents per unit. I attach some value to the lead, inasmuch as, though the lead scattered through the iron and copper pyrites is valueless, there are lodes yielding massive galena, and others where the galena might be separated from the other sulphurets, advantageously both to the miner and the smelter. Therefore—

	Dollars.
100,000 tons, containing 35 dols. in gold and silver, are worth	3,500,000
300,000 units of copper, at even 1·50 dols. per unit	450,000
300,000 units of lead, at 50 cents per unit	150,000
Absolute value of metals in the ore . .	4,100,000
Allow for loss in dressing, say 20 per cent	820,000
	3,280,000
Present yield with battery amalgamation and smelting of No. I. ore only (average of 2 years)	1,323,000
Saving under altered system	1,957,000

This saving would almost represent profits derived by miner and smelter; for the cost of crushing and concentration, were battery amalgamation supplanted by simple concentration and smelting of the whole produce of the mines, would be so much less than that of pulverising and amalgamating that this saving, added to the profit derived from the smelting of copper ores, concentrated as they then would be to 10 to 12 per cent, and purchased probably at 2·50 dols. per unit, would pay for the cost of treatment. Of course the miner would not receive from the smelter the full value of the gold and silver, but he would receive a higher percentage of their value than he now does, and thus the smelter and he would divide the increased profit between them.

As I shall show, the cost of mining and milling is now

* Raymond, Report for 1870, p. 294.

approximately 12 dols. per ton. The return of bullion derived from the 100,000 tons crushed confirms therefore, what is evident from other considerations, that the mines have, as a whole, merely paid the miner and mill-man their extravagant wages, without returning any profit on the capital invested in the mine. Could, however, the heavy loss now sustained be saved, the saving would be nearly clear profit. But, beside that, the very method which would save the waste would enable much of the third quality ore, now broken and raised at considerable cost, to be utilised. But to this subject I shall return after describing the

Smelting Works now in Operation.

One establishment—that of the Boston and Colorado Smelting Company—has for five years monopolised the smelting ores raised around Central City, and under the admirable management of Prof. Hill the enterprise has succeeded financially and metallurgically.

At present there are five calcining and three reverberatory smelting furnaces running. The ores treated are the No. I. iron and copper pyrites, and concentrated tailings, containing gold and silver. Galena ores are not sought for; but a certain amount of galena and blende is necessarily present in the mixture, and the latter is in sufficient quantity to be a source of trouble by carrying silver into the slag. A cupola furnace is employed to re-melt this zincy slag,—an expensive operation, as coke costs between 40 and 50 dols. a ton.

The fuel used in the reverberatories is wood. The calciners roast 3 tons of tailings daily, with the consumption of one cord of wood, costing 7 dols. a cord. The smelting furnaces consume about 12 cords of wood, and smelt 4 charges of 2 tons each in the twenty-four hours. The lumps of coarse ore are heap-roasted.

Professor Hill aims at getting a 40 per cent copper matt, containing 40 ozs. of gold and 400 to 600 ozs. of silver to the ton. He has always sold his matt to Vivian and Co., Swansea. His works have been of immense advantage to Gilpin County. Yet he is not in good odour with the miners generally. His scale of prices is low—lower probably than if there were a vigorous competition on the spot. Special arrangements, however, are made with good customers. But in every case the value of most of the parcels of ore is evidently guessed at, as the rough mode of sampling

employed can afford only a very vague determination of their contents. The scale of prices paid is about as follows:—

Ounces of Fine Gold per Ton of 2000 lbs.	Percentage Paid of the Value of the Gold and Copper.
10	60
9	58
8	55
7	52½
6	50
5	45
4	40
3	30
2	20

The silver in the gold ores is not generally allowed for, nor is anything paid for the copper, unless in special cases.*

Professor Hill's were not the first smelting works attempted in this district; and others have failed since the establishment of his, though not in every case through defects in the method. Mr. Wm. West erected, two years ago, smelting and sulphuric acid works, with a view of removing the zinc as sulphate by Gremm's method before smelting the ores; but the concern failed through lack of capital before getting fairly under weigh. At present Mr. West is superintending works at Golden City, about half way between Denver and Central City. The works are located there in order to be near the lignite, which occurs in thick beds along the base of the mountains, and which is delivered at the works for 4·50 dols. per ton; and also because Golden City is central to the lead ores of

* It may not be uninteresting to compare the above tariff with the price paid for gold ores, to be similarly treated, at the smelting works on the Copiopó, in Chili.

Ounces of fine Gold per cajon = 64 Spanish quintals.		Price paid per ounce.
4 ozs. per cajon, or	1 oz. 120 grs. per ton of 2000 lbs.	6·50 dols.
5 ,,	,, 1 ,, 270 ,, ,,	7·75 ,,
6 ,,	,, 1 ,, 420 ,, ,,	8·90 ,,
8 ,,	,, 2 ozs. 240 ,, ,,	9·80 ,,
10 ,,	,, 3 ,, 60 ,, ,,	10·40 ,,
12 ,,	,, 3 ,, 360 ,, ,,	10·80 ,,
14 ,,	,, 4 ,, 180 ,, ,,	11·20 ,,
16 ,,	,, 5 ,, — ,, ,,	11·70 ,,
18 ,,	,, 5 ,, 300 ,, ,,	12·30 ,,
20 ,,	,, 6 ,, 120 ,, ,,	12·90 ,,

The same works pay for silver ores, when they contain 6 marks to the cajon, or 60·60 dols. of silver to the 64 Spanish quintals—

For	6-mark ore 1·00 dol.	per mark, or	1-10th	of the value of the silver.
,,	10-mark ore 3·00 dols.	,,	3-10ths	,, ,,
,,	40-mark ore 6·00 ,,	,,	6-10ths	,, ,,
,,	100-mark ore 7·25 ,,	,,	3-4ths	,, ,,

Georgetown, and of the new mineral region of Caribou, in Boulder County. They are designed to treat argentiferous galena only. In a combined calcining and smelting furnace the galena, mixed with a suitable proportion of siliceous matter, is first roasted by being moved forward from the stack to near the fireplace over a hearth 60 feet long. Immediately behind the bridge there is a depression, into which the calcined ore is drawn, and where it is fused into a pasty mass, with pyrites tailings from Central City. This mass of silicate of lead, with some galena and sulphide of iron, is smelted, with a small percentage of metallic iron, in a cupola furnace. Separating works are being put up to desilverise the lead. Although the lignite answers admirably in the calciners, Pennsylvania coke at 40 dols. a ton is the fuel consumed in the cupola.

Another market for Gilpin County ore is being made in Idaho, about six miles from Central City, over a steep lateral mountain range, where an English company is commencing the erection of furnaces in the old Whale Mill of the Spanish Bar Silver Mining Company. The works are near enough to Central to compete with Professor Hill for the richest of the gold-bearing sulphurets of that region, which it will be found necessary to mix with the more refractory ores of Idaho. At about the same distance from Idaho, but much more easily reached, because upon the banks of the same river, is the Empire City district, which will also furnish iron and copper sulphurets; but small smelting works, owned by a Swansea firm, are already in operation at this point.

Some of the richer argentiferous galena of Gilpin County finds its way to Georgetown, where Mr. J. O. Stewart has erected and is running to good profits beautifully arranged silver reduction works of the Reese River type; but to describe them would be beyond the purpose of this article. The prices he pays are somewhat more favourable than those of Prof. Hill; but it is worthy of note that the greater satisfaction of his customers arises in great measure from the accuracy with which the sample is taken, and the certainty the seller feels of knowing what his ore really contains.

Present Financial Position of Mining and Milling, and Proposed Alteration in the Mode of Treating the Ores.

So heavy is the loss entailed on the miner by the present system of milling, that Mr. Reichenecher computes that the

"gross receipts from rock of the first class are about 36·24 per cent, and from rock of the second class about 32·13 per cent of their total assay value in gold, silver, and copper." He classes the veins now worked under two heads, the first yielding an average in gold, silver, and copper of 50·80 dols.; the second class an average of 30 dols. per ton. The first, as he shows, returns a profit to the miner; the second is worked at a loss.

Fifty tons of ore, whose contents in gold, silver, and copper is worth, say 50 dols. per ton, but in gold, which alone is saved, only 29·40 dols., will cost and yield as follows:—

	Dollars.
5 tons, worth, say 100 dols., will bring from the smelters 60 dols. per ton . .	300·00

Forty-five tons, treated in the mill, and worth in gold 29·40 dols. per ton, will yield as follows:—

	Dollars.
40 per cent of 29·40 dols. per ton, the result of battery amalgamation	529·20
4 per cent of 29·40 dols. per ton derived from the blanketings when treated in pans	52·92
Concentrated tailings, 2·25 tons at 16·94 dols.	38·12
	920·24

Cost of Mining and Milling.

	Dollars.	
Mining and carting 50 tons at 6 dols. per ton	300·00	
Milling 45 tons at 3·84 dols. . .	172·90	
Treating blanketings in pans 14½ cents per ton	6·30	
Concentrating tailings	9·81	
	——	489·01
Gross profit		431·23
From this must be deducted cost of administration, and tax of, say 18 cents per ton, amounting to, say 1·26 dols. per ton		63·09
Nett profits		368·14

Or, 7·36 dols. per ton.

Veins of the second class containing 30 dols. in gold, silver, and copper, but not over 21 dols. in gold alone, it is evident

can only be worked at a loss, which is calculated by Mr. Reichenecher at 1·73 dols. per ton.

Most of the mines which have been kept open on the lodes previously enumerated are of the first class. Many, of course, have yielded much higher percentage ore than the average; many more have intermitted between poverty and richness; and there is many an isolated mine, like the California on the Flack Lode, which has for many consecutive months by its large returns effected the prosperity of the whole region. There are, moreover, first class claims held by wealthy men who can afford to wait, and who have kept them closed for years, sure of the introduction sooner or later of more economical methods of treatment. Of each there are several on the Bates Lode. The best evidence of the unparalleled richness of these mines is that, despite the loss of 66 per cent of their mineral, so many have been for years worked to advantage. A comparison of their produce with those of other gold-producing countries affords further proof of this.* The average value of 500,000 tons of Australian gold quartz was 16·78 dols.; the average value of ore raised in eight counties in California from 30 mines, including the richest, is, per ton, 23·50 dols.; while 1,760,050 tons from the Morro Velho mines, Brazil, yielded only 8·20 dols. per ton, and yet the ores of Gilpin County must yield 25 dols. in gold to cover cost of extraction and milling alone. If the character of the ore is so peculiar as to defy all known methods of economical treatment, the mines must be left to their inevitable fate. But there is no reason to apprehend such a gloomy future.

The remedy evidently lies in mechanical concentration of the second and third class ores, the abandonment altogether of battery amalgamation, and the smelting of the whole produce.

The ore should be carefully assorted by hand, and a separation made not only of first class, as at present from the poorer vein stuff, but of the iron and copper pyrites from the galena and blende.

First class ore, as at present, is fit for the furnace, and can be roasted either in heaps, or, better still, in kilns; for it is a serious waste of capital to have 100,000 to 200,000 dols. worth of ore lying in roast heaps for months, when the amount might be returned in as many weeks were kilns employed. Second and third class ore, the former of which alone is now serviceable, might both be crushed and

* Baker's Pamphlet, p. 15.

concentrated. The gangue is generally soft and light, and easily separable from its mineral contents; and the mineral is not, as a general rule, distributed in such minute particles through the mass as to necessitate crushing finer than 1-6th to 1-8th of an inch, in order to obtain a very perfect disengagement of the one from the other. The coarse grains should be concentrated in automatic hutches. It is possible that Messrs. Huet and Geyler's hutches* would separate not only the mineral from the earthy matter, but as the toppings flow from hutch to hutch effect a certain separation of the iron and copper pyrites from the blende and galena. These hutches recommend themselves also by their compactness, and being built entirely of iron. The slime concentration would doubtless be best effected on Rittinger lateral percussion tables, which would certainly not only concentrate, but separate the concentrate into parcels of different specific gravity; but the machine requires for successful working too close attention to so many details to be efficient in the hands of Colorado ore-dressers. Buddles therefore—concave buddles for the coarse, and convex buddles for the fine slimes—would be the most suitable machines. If third class ore, which will not bear expensive carriage, is to be utilised, the concentrating works would need to be at the mines. Water could be delivered to most mines from the Consolidated Ditch. The charges are now high, but it is expected they will be reduced to 10 cents per miner's inch per day = 2274 cubic feet of water.

Dry concentration is strongly advocated, but where water is accessible it will in most cases be better to adhere to the well-understood system of water dressing.

If the concentration were as carefully conducted as it is in the best establishments of England and the Continent, the result should be as favourable. In Hungary the allowance for loss is 15 per cent. Allow that it would be 20 per cent in Colorado, and that the concentrate would contain four times as much mineral as the crude ore. If, therefore, the mineral contained 1 oz. of gold and 1·5 per cent of copper, the concentrate would contain, after making allowance for loss, 4 ozs. of gold and 6 per cent of copper. I leave the silver and lead out of the calculation. If the galena and blende can be separated from the iron and copper pyrites the galena will be an additional source of profit; if not, the cost of smelting the refractory mixture and the

* Huet and Geyler, 46, Rue de la Victoire, manufacturers of the Cribles Rapides à Deux et Quatre Compartiments.

loss of silver in the slag will be so great as to reduce notably the value of the silver.

The probable cost of treating will, therefore, be—

	Dollars.
Mining and hauling 50 tons at 6 dols. per ton	300
Handpicking and concentrating 50 tons at 2 dols.	100
	400

Value of gold and silver in the concentrate—

	Dollars.	
10 tons of concentrate containing 40 ozs. of gold	800	
10 tons of concentrate containing 60 units of copper	300	
		1100

The smelter should pay for ore containing 4 ozs. of gold, 6 per cent of copper, and probably 20 to 40 ozs. of silver, at least 60 per cent of the value of the gold and 50 per cent of the value of the copper.

Therefore the receipts of the miner would be—

	Dollars.
60 per cent of the value of 40 ozs. of gold .	480
50 per cent of the value of the copper . . .	150
	630
And as the cost of producing and concentrating would be	400
The profit on 50 tons of 1 oz. gold ore would be	230

Or 4·60 dols. per ton.

This calculation supposes that there is no No. 1 ore in the vein stuff. As the custom mills charge only 3·84 dols. for stamping and amalgamating a ton of ore, the allowance of 2·00 dols. for crushing and concentrating is ample. Moreover, the smelting would doubtless be done more cheaply were there vigorous competition. But this cannot be looked for till smelters can count with certainty on a steady and abundant supply of suitable ore, which will only be forthcoming when the whole produce of the mines passes through their hands, and not the No. 1 ore only.

From mines now open 1000 tons a day of 20-dollar ore could be at once produced; and there are second class mines innumerable which under existing modes of treatment are

valueless and closed, which could quadruple that yield if it were shown that a 20-dollar ore could be mined to a profit.

Several English companies are now entering on active mining operations in Gilpin County, and it is to be hoped they will inaugurate a new system. This they are the more likely to do, as the properties they have purchased are not hampered with old stamp mills.

III. CONDITION OF THE MOON'S SURFACE.

By RICHARD A. PROCTOR, B.A. (Cambridge),
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IF the study of our earth's crust—or the science of Geology—is capable of throwing some degree of light on the past condition of other members of the solar system, the study of those other orbs seems capable of at least suggesting useful ideas concerning the past condition of our earth. There are members of the solar system respecting which it may reasonably be inferred that they are in an earlier stage of their existence than the earth. Jupiter and Saturn, for instance, would seem—so far as observation has extended—to be still in a condition of intense heat, and still the seat of forces such as were once probably at work within our earth. We see these planets enwrapped, to all appearance, within a double or triple coating of clouds, and we are compelled to infer, from the behaviour of these clouds, that they are generated by forces belonging to the orb which they envelope; we have, also, every reason which the nature of the case can afford to suppose that our own earth was once similarly cloud-enveloped. We can scarcely imagine that in the long-past ages, when the igneous rocks were in the primary stages of their existence, the air was not loaded heavily with clouds. We may, then, regard Jupiter and Saturn as to some degree indicating the state of our own earth at a long-past epoch of her existence. On the other hand, it has been held, and not without some degree of evidence in favour of the theory, that in our moon we have a picture of our earth as she will be at some far-distant future date, when her period of rotation has been forced into accordance with the period of the moon's revolution round the earth, when the internal heat of the earth's globe

has been radiated almost wholly away into space, and when her oceans and atmosphere have disappeared through the action of the same circumstances (whatever they may be) which have caused the moon to be air-less and ocean-less. But whether we take this view of our earth's future, or whether we consider that her state has been from the beginning very different from that of the moon, it nevertheless remains probable that we see in our moon a globe which has passed through a much greater proportion of its history (so to speak) than our earth; and accordingly the study of the moon's condition seems capable of giving some degree of information as to the future (possibly also as to the past) of our earth.

I wish, in the present paper, to consider the moon's condition from a somewhat different point of view than has commonly been adopted. It appears to me that the study fo the moon's surface with the telescope, and the consideration of the various phenomena which give evidence on the question whether air or water exist anywhere upon or within her, have not as yet led to any satisfactory inferences as to her past history. We see the traces of tremendous sublunarian disturbances (using the word "sublunarian," here and elsewhere, to correspond to the word "subterranean" used with reference to the earth), and we find some features of resemblance between the effects of such disturbances and those produced by the subterranean forces of our earth; but we find also as marked signs of distinction between the features of the lunar and terrestrial crusts. Again, comparing the evidences of a lunar atmosphere with those which we should expect if an atmosphere like our own surrounded the moon, we are able to decide, with some degree of confidence, that the moon has either no atmosphere or one of very limited extent. But there our knowledge comes to an end; nor does it seem likely that, by any contrivances man can devise, the further questions which suggest themselves respecting the moon's condition can be answered by means of observation.

But there are certain considerations respecting the moon's past history which seem to me likely, if duly weighed, to throw some light on the difficult problems presented by the moon.

In the first place, it is to be noted that the peculiar relation between the moon's rotation and revolution possesses a meaning which has not hitherto, so far as I know, been attended to. We know that *now* there is an absolutely perfect agreement between the moon's rotation and revolu-

tion, in this respect—that her mean period of rotation on her axis is exactly equal to her mean period of revolution. (Here either sidereal rotation and revolution or synodical rotation and revolution may be understood, so long as both revolution and rotation are understood to be of the same kind). I say "mean period of rotation," for although as a matter of fact it is only the revolution which is subject to any considerable variation, the rotation also is not perfectly uniform. We know, furthermore, that if there had been, long ago, a *near* agreement between the mean rotation and revolution, the present exact agreement would have resulted, through the effects of the mutual attractions of the earth and moon. But, so far as I know, astronomers have not yet carefully considered the question whether that close agreement existed from the beginning, or was the result of other forms of action than are at present at work. If it existed from the beginning, that is from the moon's first existence as a body independent of the earth, it is a matter requiring to be explained, as it implies a peculiar relation between the moon and earth before the present state of things existed. If, on the contrary, it has been brought about by the amount of action which is now gradually reducing the earth's rotation period, we have first of all to consider that an enormous period of time has been required to bring the moon to her present condition in this respect, and, moreover, that either an ocean existed on her surface or that her crust was once in so plastic a condition as to be traversed by a tidal wave resembling, in some respects, the tidal wave in our own ocean. This, at any rate, is what we must believe if we suppose, first, that the main cause of the lengthening of the terrestrial day is the action of the tidal wave as a sort of brake on the earth's rotating globe, and, secondly, that a similar cause produced the lengthening of the moon's day to its present enormous duration. It may be, as we shall presently see, that other causes have to be taken into account in the moon's case.

Now we are thus, either way, brought to a consideration of that distant epoch when—according to the nebular theory, or any admissible modification thereof—the moon was as yet non-existent as an orb distinct from the earth. We must suppose, on one theory, that the moon was at that time enveloped in the nebulous rotating spheroid out of which the earth was to be formed, she herself (the moon) being a nebulous sub-spheroid within the other, and so far coerced by the motion of the other that her longer axis partook in its motion of rotation. Unquestionably in that

case, as the terrestrial spheroid contracted and left the other as a separate body, this other, or lunar spheroid, would exhibit the kind of rotation which the moon actually possesses. On the other theory, we should be led to suppose that primarily the lunar spheroid rotated independently of its revolution; but that the earth's attraction acting on the outer shells, after they had become first fluid and then (probably) viscous, produced waves travelling in the same direction as the rotation, but with a continual brake-action, tending slowly to reduce the rotation until it had its present value, when dynamical equilibrium would be secured.

But, as I have said, in either case we must trace back the moon's history to an epoch when she was in a state of intense heat. And it seems to me that we are thus led to notice that the development of the present state of things in the moon must have taken place during an era in the history of the solar system differing essentially from that which prevailed during the later and better-known geological eras of our own earth. Our moon was *shaped*, so to speak, when the solar system itself was young, when the sun may have given out a much greater degree of heat than at present, when Saturn and Jupiter were brilliant suns, when even our earth and her fellow minor planets within the zone of asteroids were probably in a sun-like condition. Putting aside all hypothesis, it nevertheless remains clear that, to understand the moon's present condition, we must form some estimate of the probable condition of the solar system in distant eras of its existence; for it was in such eras, and not in an era like the present, that she was modelled to her present figure.

It appears to me that we are thus, to some extent, freed from a consideration which has proved a difficulty to many who have theorised respecting the moon. It has been said that the evidence of volcanic action implies the existence, at least when that action was in progress, of an atmosphere capable of supporting combustion,—in other words, an atmosphere containing oxygen, for other forms of combustion than those in which oxygen plays a part may here be dismissed from consideration. But the fiery heat of the moon's substance may have been maintained (in the distant eras to which we are now referring the formation of her crust) without combustion. Taking the nebular hypothesis as it is commonly presented, the moon's globe may have remained amid the intensely hot nebulous spheroid (which was one day to contract, and so form the globe of the earth) until the nebula left it to cool thenceforth rapidly to its present

state. Whatever objections suggest themselves to such a view are precisely the objections which oppose themselves to the simple nebular hypothesis, and may be disposed of by those who accept that hypothesis. But better, to my view, it may be reasoned, that the processes of contraction and of the gathering in of matter from without, which maintained the heat of the nebulous masses, operated to produce all the processes of disturbance which brought the moon to her present condition, and that thus there was not necessarily any combustion whatever. Indeed, in any case, combustion can only have commenced when the heat had been so far reduced that any oxygen existing in the lunar spheroid would enter into chemical combination with various components of the moon's glowing substance. If there were no oxygen (an unlikely supposition, however), the moon's heat would nevertheless have been maintained so long as meteoric impact on the one hand, and contraction of the moon's substance on the other, continued to supply the requisite mechanical sources of heat-generation. In this case there would not necessarily have been any gaseous or vapourous matter, other than the matter retained in the gaseous condition by intensity of heat, and becoming first liquid and afterwards solid, so soon as the heat was sufficiently reduced.

It must here be considered how far we have reason to believe that the heat of the various members of the solar system—including the moon and other secondary bodies—was originally produced, and thereafter maintained, by collisions; because it is clear that, as regards the surface contour of these bodies, much would depend on this circumstance. There would be a considerable difference between the condition of a body which was maintained at a high temperature for a long period, and eventually cooled, but slowly, under a continual downfall of matter, and that of a body whose heat was maintained by a process of gradual contraction. It is true that in the case of a globe like the earth, whose surface was eventually modelled and re-modelled by processes of a totally different kind, by deposition and denudation, by wind and rain, river-action and the beating of seas, the signs of the original processes of cooling would to a great extent disappear; but if, as we are supposing in the case of the moon, there was neither water nor air (at least in sufficient quantity to produce any effect corresponding to those produced by air and water on the earth), the principal features of the surface would depend largely on the

conditions under which the process of cooling began and proceeded.

Now here I must recall to the attention of the reader the reasoning which I have made use of in my "Other Worlds than Ours," to show that, in all probability, our solar system owed its origin rather to the gathering of matter together from outer space than to the contraction of a rotating nebulous mass. It is there shown, and I think that the consideration is one which should have weight in such an inquiry, that there is nothing in the nebular hypothesis of Laplace to account in any degree for the peculiarities of detail presented by the solar system. That theory explains the revolution of the members of the solar system in the same direction, their rotation in the same direction, the approach to circularity of the orbits, and their near coincidence with the mean plane of the system; but it leaves altogether unexplained the different dimensions of the primary members of the solar system, the apparent absence of law and order in their axial tilt, and the inclination of the orbits of their satellite families. In particular, the remarkable difference which exists between the outer family of planets,—the giant orbs, Jupiter, Saturn, Uranus, and Neptune,—and the inner family of small planets,—Mars, the Earth, Venus, and Mercury,—is left wholly unexplained. Nor can one recognise in the nebular hypothesis any reason whatever for the comparative exuberance of orb-forming activity in the outer family, and particularly in the two planets lying next to the zone of asteroids, and the poverty of material which is exhibited within the minor family of planets. All these circumstances appear to be explained satisfactorily when we regard the solar system as formed by the gathering in from outer space of materials once widely scattered. We can see that in the neighbourhood of the great primary centre there would be indeed a great abundance of gathered and gathering matter, but that, owing to the enormous velocities in that neighbourhood, subordinate centres of attraction would there form slowly, and acquire but moderate dimensions. Outside a certain distance there would be less matter, but a far greater freedom of aggregation; there we should find the giant secondary centres, and we should expect the chief of these to lie inwards, as Jupiter and Saturn, while beyond would be orbs vast indeed, but far inferior to these planets. And we can readily see that the border region between the family of minor planets and the family of major planets would be one where the formation of a planet would be rendered unlikely; here, therefore, we

should look for the existence of a zone of small bodies like the asteroids. I touch on these points to show the kind of evidence (elsewhere given at length) on which I have based my opinion that the solar system had its birth, and long maintained its fires, under the impact and collisions of bodies gathered in from outer space.

According to this view, the moon, formed at a comparatively distant epoch in the history of the solar system, would have not merely had its heat originally generated for the most part by meteoric impact, but while still plastic would have been exposed to meteoric downfalls, compared with which all that we know, in the present day, of meteor-showers, aërolitic masses, and so on, must be regarded as altogether insignificant. It would be to such downfall mainly that the maintenance of the moon's heat would at that time be due, though, as we shall presently see, processes of contraction must have not only supplemented this source of heat-supply, but must have continued to maintain the moon's heat long after the meteoric source of heat had become comparatively ineffective.

Now, I would notice in passing that here we *may* find an explanation of the agreement between the moon's rotation period and her period of revolution. It is clear that under the continuous downfall of meteoric matter in that distant era, the moon must have been in a process of actual growth. She is indeed growing *now* from the same cause; and so is the earth: but such growth must be regarded as infinitesimally small. In the earlier periods of the moon's history, on the contrary, the moon's growth must have progressed at a comparatively rapid rate. Now this influx of matter must have resulted in a gradual reduction of the moon's rate of rotation, if (as we must suppose) the moon gathered matter merely by chance collisions. In the case of a globe gathering in matter by its own attractive power as the sun does, for instance, the arriving matter may (owing to the manner in which the process is effected) serve to maintain and even to increase the rate of rotation; but in the case of a subordinate body like the moon we must suppose that all effects acting on the rotation would be about equally balanced, and that the sole really effective result would be the increase of the moon's bulk, and the consequent diminution of her rotation rate. Now, if this process continued until the rotation rate had nearly reached its present value, the earth's attraction would suffice not merely to bring the rate of rotation precisely to its present value, but to prevent its changing (by the continuance of the process) to a smaller

value. It may be added that the increase in the moon's rate of revolution, as she herself and the earth both grew under meteoric downfall towards their present dimensions, would operate in a similar way,—it would tend to bring the moon's rate of revolution and her rate of rotation towards that agreement which at present exists.

If we attempt to picture the condition of the moon in that era of her history when first the process of downfall became so far reduced in activity as to permit of her cooling down, we shall be tempted, I believe, to consider that some of the more remarkable features of her globe had their origin in that period. It may seem, indeed, at a first view, too wild and fanciful an idea to suggest that the multitudinous craters on the moon, and especially the smaller craters revealed in countless numbers when telescopes of high power are employed, have been caused by the plash of meteoric rain,—and I should certainly not care to maintain that as the true theory of their origin; yet it must be remembered that no plausible theory has yet been urged respecting this remarkable feature of the moon's surface. It is impossible to recognise a real resemblance between any terrestrial feature and the crateriferous surface of the moon. As blowholes, so many openings cannot at any time have been necessary, whatever opinion we may form as to the condition of the moon's interior and its reaction upon the crust. Moreover, it should be remembered that our leading seismologists regard water as absolutely essential to the production of volcanic disturbance (the only form of disturbance which on our earth leads to the formation of cup-shaped openings). If we consider the explanation advanced by Hooke, that these numerous craters were produced in the same way that small cup-shaped depressions are formed when thick calcareous solutions are boiled and left to cool, we see that it is inadequate to account for lunar craters, the least of which (those to which Mr. Birt has given the name of craterlets) are at least half a mile in diameter. The rings obtained by Hooke were formed by the breaking of surface bubbles or blisters,* and it is impossible for such bubbles to be formed on the scale of the lunar craters. Now so far as the smaller craters are concerned, there is nothing incredible in the supposition that they were

* "Presently ceasing to boil," he says of alabaster, "the whole surface will appear covered all over with small pits, exactly shaped like those of the moon." "The earthy part of the moon has been undermined," he proceeds, "or heaved up by eruptions of vapour, and thrown into the same kind of figured holes as the powder of alabaster."

due to meteoric rain falling when the moon was in a plastic condition. Indeed, it is somewhat remarkable how strikingly certain parts of the moon resemble a surface which has been rained upon while sufficiently plastic to receive the impressions, but not too soft to retain them. Nor is it any valid objection to this supposition, that the rings left by meteoric downfall would only be circular when the falling matter chanced to strike the moon's surface squarely; for it is far more probable that even when the surface was struck very obliquely and the opening first formed by the meteoric mass or cloud of bodies was therefore markedly elliptic, the plastic surface would close in round the place of impact until the impression actually formed had assumed a nearly circular shape.

Before passing from this part of my subject, I would invite attention to the aspect of the half moon as presented in the photograph illustrating this paper (see Frontispiece).* It will be seen that the multitudinous craters near the top of the picture (the southern part of the moon†) are strongly suggestive of the kind of process I have referred to, and that, in fact, if one judged solely by appearances, one would be disposed to adopt somewhat confidently the theory that the moon had had her present surface craters chiefly formed by meteoric downfalls during the period of her existence when she was plastic to impressions from without. I am, however, sensible that the great craters under close telescopic scrutiny by no means correspond in appearance to what we should expect if *they* were formed by the downfall of great masses from without. The regular, and we may almost say battlemented, aspect of some of these craters, the level floor, and the central peaks so commonly recognised, seem altogether different from what we should expect if a great mass fell from outer space upon the moon's surface. It is indeed just possible that under the tremendous heat

* This photograph is interesting as the work of the Great Melbourne reflector. It was taken directly of its present size, and in this respect differs from all others of the same size, since, hitherto the negatives taken have been small.

† Owing to the fact that this photograph has been taken with a Newtonian reflector, we have not the same kind of inversion as in the case of photographs taken with refractors. In the latter case all that is necessary to cause the picture to represent the moon as we see her, is simply to hold the picture upside down; but the photograph illustrating this paper will only resemble the half moon as she actually appears (at the time of first quarter, the epoch of the photograph) by holding the picture inverted before a looking-glass. The picture would also show rightly if inverted and then looked at from behind, supposing the method of mounting such that the picture can be seen from behind when held up between the eye and the light. At present I do not know whether this will be the case or not.

generated by the downfall a vast circular region of the moon's surface would be rendered liquid, and that in rapidly solidifying while still traversed by the ring-waves resulting from the downfall, something like the present condition would result. Or we might suppose that the region liquefied through the effects of the shock was very much larger than the meteoric mass; and that while a wave of disturbance travelled outwards from the place of impact to be solidified (owing to rapid radiation of heat) even as it travelled, a portion of the liquid interior of the moon forced its way through the opening formed by the falling mass. But such ideas as these require to be supported by much stronger evidence than we possess before they can be regarded as acceptable. I would remark, however, that nothing hitherto advanced has explained at all satisfactorily the structure of the great crateriform mountain ranges on the moon. The theory that there were once great lakes seems open to difficulties at least as grave as the one I have just considered, and to this further objection, that it affords no explanation of the circular shape of these lunar regions. On the other hand, Sir John Herschel's account of the appearance of these craters is not supported by any reasoning based on our knowledge of the actual circumstances under which volcanic action proceeds in the case of our own earth. "The generality of the lunar mountains," he says, "present a striking uniformity and singularity of aspect. They are wonderfully numerous, occupying by far the larger portion of the surface, and almost universally of an exact circular or cup-shaped form, foreshortened, however, into ellipses towards the limb; but the larger have for the most part flat bottoms within, from which rises centrally a small, steep, conical hill. They offer, in short, in its highest perfection the true volcanic character, as it may be seen in the crater of Vesuvius; and, in some of the principal ones, decisive marks of volcanic stratification, arising from successive deposits of ejected matter, may be clearly traced with powerful telescopes. What is, moreover, extremely singular in the geology of the moon is, that although nothing having the character of seas can be traced (for the dusty spots which are commonly called seas, when closely examined, present appearances incompatible with the supposition of deep water), yet there are large regions perfectly level, and apparently of a decided alluvial character?"

It is obvious that in this description we have, besides those features of volcanic action which might perhaps be expected on the moon, a reference to features essentially

terrestrial. Alluvial deposits can have no existence, for example, save where there are rivers and seas, as well as an atmosphere within which clouds may form, whence rain may be poured upon the surface of wide land regions. It is not going too far to say that we have the clearest evidence to show that in the moon none of these conditions are fulfilled. Whether in former ages lunar oceans and seas and a lunar atmosphere have existed, may be a doubtful point; but it is certain that all the evidence we have is negative, save only those extremely doubtful signs of glacier action recognised by Prof. Frankland. I venture to quote from Guillemin's "Heavens" a statement of Frankland's views, in order that the reader may see on how slender a foundation hypotheses far more startling than the theory I have suggested have been based by a careful reasoner and able physicist. "Prof. Frankland believes," says the account, "and his belief rests on a special study of the lunar surface, that our satellite has, like its primary, also passed through a glacial epoch, and that several, at least, of the *valleys*, *rills*, and *streaks* of the lunar surface are not improbably due to former glacial action. Notwithstanding the excellent definition of modern telescopes, it could not be expected that other than the most gigantic of the characteristic details of an ancient glacier-bed would be rendered visible. What, then, may we expect to see? Under favourable circumstances the terminal moraine of a glacier attains enormous dimensions; and consequently of all the marks of a glacier valley this would be the one most likely to be first perceived. Two such terminal moraines, one of them a double one, have appeared to observers to be traceable upon the moon's surface. The first is situated near the termination of the remarkable streak which commences near the base of Tycho, and passing under the south-eastern wall of Bullialdus, into the ring of which it appears to cut, is gradually lost after passing Lubiniezky. Exactly opposite this last, and extending nearly across the streak in question, are two ridges forming the arcs of circles whose centres are not coincident, and whose external curvature is towards the north. Beyond the second ridge a talus slopes gradually down northwards to the general level of the lunar surface, the whole presenting an appearance reminding the observer of the concentric moraines of the Rhône glacier. These ridges are visible for the whole period during which that portion of the moon's surface is illuminated; but it is only about the third day after the first quarter, and at the corresponding phase of the waning moon, when the sun's

rays, falling nearly horizontally, throw the details of this part of the surface into strong relief; and these appearances suggest this explanation of them. The other ridge answering to a terminal moraine, occurs at the northern extremity of that magnificent valley which runs past the eastern edge of Rheita."

Here are two lunar features of extreme delicacy, and certainly not incapable of being otherwise explained, referred by Frankland to glacier action. It need hardly be said that glacial action implies the existence of water and an atmosphere on the moon,—and not only so, but there must have been extensive oceans and an atmosphere nearly equal in density to that of our own earth, if the appearances commented upon by Frankland were due to glacial action. It is admitted by Frankland, of course, that there is now no evidence whatever of the presence of water, "but, on the contrary, all selenographical observations tend to prove its absence. Nevertheless," proceeds the account from which I have already quoted, "the idea of former aqueous agency in the moon *has received almost universal acceptation*" (the italics are mine). "It was entertained by Gruithuisen and others. But, if water at one time existed on the surface of the moon, whither has it disappeared? If we assume, in accordance with the nebular hypothesis, that the portions of matter composing respectively the earth and the moon once possessed an equally elevated temperature, it almost necessarily follows that the moon, owing to the comparative smallness of her mass, would cool more rapidly than the earth; for whilst the volume of the moon is only about 1-49th (and its mass, it might be added, only about 1-81st part), its surface is nearly 1-13th that of the earth. This cooling of the mass of the moon must, in accordance with all analogy, have been attended with contraction, which can scarcely be conceived as occurring without the development of a cavernous structure in the interior. Much of this cavernous structure would doubtless communicate, by means of fissures, with the surface; and thus there would be provided an internal receptacle for the ocean, from the depths of which even the burning sun of the long lunar day would be totally unable to dislodge more than traces of its vapour. Assuming the solid mass of the moon to contract on cooling at the same rate as granite, its refrigeration though only 180° F. would create cellular space equal to nearly $14\frac{1}{2}$ millions of cubic miles, which would be more than sufficient to engulf the whole of the lunar oceans, supposing them to bear the same proportion to the mass of the moon as our own oceans bear to that of the earth."

The great objection to this view of the moon's past history consists in the difficulty of accounting for the lunar atmosphere. It must be remembered that owing to the smallness of the moon's mass, an atmosphere composed in the same way as ours would have a much greater depth compared with its density at the mean level of the moon's surface than our atmosphere possesses compared with its pressure at the sea-level. If there were exactly the same quantity of air above each square mile of the moon's surface as there is above each square mile of the earth's surface, the lunar air would not only extend to a much greater height than ours, but would be much less dense at the moon's surface. The atmospheric pressure would in that case be about 1-6th that at our sea-level, and instead of the lower half of such an atmosphere (that is, the lower half in actual quantity of air) lying within a distance of about $3\frac{1}{2}$ miles from the mean surface, as in the case of our earth, it would extend to a distance of about 22 miles from the surface. Now this reasoning applies with increased force to the case of an atmosphere contained within the cavernous interior of the moon; for there the pressure due to the attraction of the moon's mass would be reduced. It is very difficult to conceive that under such circumstances room would not only exist for lunar oceans, but for a lunar atmosphere occupying, one must suppose, a far greater amount of space even before their withdrawal into these lunar caverns, and partially freed from pressure so soon as such withdrawal had taken place. That the atmosphere should be withdrawn so completely that no trace of its existence could be recognised does certainly appear very difficult to believe, to say the least.

Nevertheless, it is not to be forgotten that so far as terrestrial experience is concerned water is absolutely essential to the occurrence of volcanic action. If we are to extend terrestrial analogies to the case of our moon, notwithstanding the signs that the conditions prevailing in her case have been very different from those existing in the case of our earth, we are bound to recognise at least the possibility that water once existed on the moon. Moreover, it must be admitted that Professor Frankland's theory seems to accord far better with lunar facts than any of the others which have been advanced to account for the disappearance of all traces of water or air. The theory that oceans and an atmosphere have been drawn to the farther side of the moon cannot be entertained when due account is taken of the range of the lunar librations. Sir J. Herschel, indeed,

once gave countenance to that somewhat *bizarre* theory; but he admitted in a letter addressed to myself, that the objection I had based on the circumstances of libration was sufficient to dispose of the theory. The hypothesis that a comet had whisked away the lunar oceans and atmosphere does not need serious refutation; and it is difficult to see how the theory that lunar seas and lunar air have been solidified by intense cold can be maintained in presence of the fact that experiments made with the Rosse mirror indicate great intensity of heat in the substance of those parts of the moon which have been exposed to the full heat of the sun during the long lunar day.

If there ever existed a lunar atmosphere and lunar seas, then Prof. Frankland's theory seems the only available means of accounting for their disappearance. Accordingly we must recognise the extreme interest and importance of telescopic researches directed to the inquiry, whether any features of the moon's surface indicate the action of processes of *weathering,* whether the beds of lunar rivers can anywhere be traced, whether the shores of lunar seas can be recognised by any of those features which exist round the coast-lines of our own shores.

One circumstance may be remarked in passing. If the multitudinous lunar craters were formed before the withdrawal of lunar water and air into the moon's interior, it is somewhat remarkable that the only terrestrial features which can be in any way compared with them should be found in regions of the earth which geologists regard as among those which certainly have not been exposed to denudation by the action of water. Thus Sir John Herschel, speaking of the extinct volcanoes of the Puy de Dôme, remarks that here the observer sees "a magnificent series of volcanic cones, fields of ashes, streams of lava, and basaltic terraces or platforms, proving the volcanic action to have been continued for countless ages before the present surface of the earth was formed; here can be seen a configuration of surface quite resembling what telescopes show in the most volcanic districts of the moon; for half the moon's face is covered with unmistakable craters of extinct volcanoes." But Lyell, speaking of the same volcanic chains, describes them as regions "where the eruption of volcanic matter has taken place in the open air, and where the surface has never since been subjected to great aqueous denudation." If all the craters on the moon belonged to one epoch, or even to one era, we might regard them as produced during the withdrawal of the lunar oceans within the still heated substance

of our satellite. But it is manifest that the processes which brought the moon's surface to its present condition must have occupied many ages, during which the craters formed earliest would be exposed to the effects of denudation, and to other processes of which no traces can be recognised. It is not likely, however, that the withdrawal of the lunar oceans into the moon's cavernous interior can have taken place suddenly; up to a certain epoch the entry of the waters within the moon's mass would be impossible, owing to the intense heat, which, by maintaining the plasticity of the moon's substance, would prevent the formation of cavities and fissures, while any water brought into contact with the heated interior would at once be vaporised, and driven away. But when once a condition was attained which rendered the formation of cavities possible, the contraction of the moon's substance would lead to the gradual increase of such cavities, and so, as time proceeded, room would be found for all the lunar oceans.

We are next led to the inquiry whether the contraction of the moon's substance may not have played the most important part of all, in producing those phenomena of disturbance which are presented by the moon's surface. Quite recently the eminent seismologist Mallet has propounded a theory of terrestrial volcanic energy, which not only appears to account—far more satisfactorily than any hitherto adopted—for the phenomena presented by the earth's crust, but suggests considerations which may be applied to the case of the moon, and in fact are so applied by Mallet himself. It behoves us to inquire very carefully into the bearing of this theory upon the subject of lunar seismology, and therefore to consider attentively the points in which the theory differs from those hitherto adopted.

Mallet dismisses first the chemical theory of volcanic energy, because all known facts tend to show that the chemical energies of the materials of our globe were almost wholly exhausted prior to the consolidation of its surface. This may be regarded as equally applicable to the case of the moon. It is difficult to see how the surface of the moon can have become consolidated while any considerable portion of the chemical activity of her materials remained unexhausted.

"The mechanical theory," proceeds Mallet, "which finds in a nucleus still in a state of liquid fusion a store of heat and of lava, &c., is only tenable on the admission of a very thin solid crust; and even through a crust but 30 miles thick, it is difficult to see how surface-water is to gain access

to the fused nucleus; *yet without water there can be no volcano.* More recent investigation on the part of mathematicians has been supposed to prove that the earth's crust is not thin." He proceeds to show that, without attaching any great weight to these mathematical calculations, there are other grounds for believing that the solid crust of the earth is of great thickness, and that "although there is evidence of a nucleus much hotter than the crust, there is no certainty that any part of it remains liquid; but if so, it is in any case too deep to render it conceivable that surface-water should make its way down to it. The results of geological speculation and of physico-mathematical reasoning thus oppose each other; so that some source of volcanic heat closer to the surface remains to be sought. The hypothesis to supply this, proposed by Hopkins and adopted by some, viz., of isolated subterranean lakes of liquid matter, in fusion at no great depth from the surface, remaining fused for ages, surrounded by colder and solid rock, and with (by hypothesis) access of surface-water, seems feeble and unsustainable."

Now in some respects this reasoning is not applicable to the moon, at least so far as real evidence is concerned; though it is to be noticed that, if a case is made out for any cause of volcanic action on the earth, we are led by analogy to extend the reasoning (or at least its result) to the case of the moon. But it may be remarked that the solidification of the moon's crust must have proceeded at a more rapid rate than that of the earth's, while the proportion of its thickness to the volume of the fused nucleus would necessarily be greater for the same thickness of the crust. The question of the access of water brings us to the difficulty already considered,—the inquiry, namely, whether oceans originally existed on the moon. For the moment, however, we forbear from considering whether Mallet's reasoning must necessarily be regarded as inapplicable to the moon if it should be admitted that there never were any lunar oceans.

We come now to Mallet's solution of the problem of terrestrial volcanic energy.

We have been so long in the habit of regarding volcanoes and earthquakes as evidences of the earth's subterranean forces,—as due, in fact (to use Humboldt's expression), to the reaction of the earth's interior upon its crust,—that the idea presents itself at first sight as somewhat startling, that all volcanic and seismic phenomena, as well as the formation of mountain ranges, have been due to a set of cosmical

forces called into play by the *contraction* of our globe. According to the new theory, it is not the pressure of matter under the crust outwards, but the pressure of the earth's crust inwards, which produces volcanic energy. Nor is this merely substituting an action for reaction, or *vice versa.* According to former views, it was the inability of the crust to resist pressure from within which led to volcanic explosions, or which produced earthquake throes where the safety-valve provided by volcanoes was not supplied. The new theory teaches, in fact, that it is a deficiency of internal resistance, and not an excess, which causes these disturbances of the crust. "The contraction of our globe," says Mallet,* "has been met, from the period of its fluidity to its present state,—first, by deformation of the spheroid, forming generally the ocean-basins and the land; afterwards by the foldings over and elevations of the thickened crust into mountain-ranges, &c.; and, lastly, by the mechanism which gives rise to volcanic actions. The theory of mountain elevation proposed by C. Prévost was the only true one,—that which ascribes this to tangential pressures propagated through a solid crust of sufficient thickness to transmit them, these pressures being produced by the relative rate of contraction of the nucleus and of the crust; the former being at a higher temperature, and having a higher coefficient of contraction for equal loss of heat, tends to shrink away from beneath the crust, leaving the latter partially unsupported. This, which during a much more rapid rate of cooling from higher temperature of the whole globe, and from a thinner crust, gave rise in former epochs to mountain-elevation, in the present state of things gives rise to volcanic heat." By the application of a theorem of Lagrange, Mr. Mallet proves that the earth's solid crust, however great may be its thickness, "and even if of materials far more cohesive and rigid than those of which we must suppose it to consist, must, if even to a very small extent left unsupported by the shrinking away of the nucleus, crush up in places by its own gravity, and by the attraction of the nucleus. This is actually going on; and in this partial crushing," at places or depths dependent on the material and on conditions which Mr. Mallet points out, he discerns "the true cause of volcanic heat.† As the solid

* I quote throughout from an abstract of Mallet's paper in the "Philosophical Magazine" for December, 1872. The words are probably, for the most part, Mallet's own; but I have not the original paper by me for reference. I believe, however, that the abstract is from his own pen.

† "In order to test the validity of his theory by contact with known facts" (says the "Philosophical Magazine"), "Mr. Mallet gives in detail two im-

crust sinks together to follow down after the shrinking nucleus, the *work* expended in mutual crushing and dislocation of its parts is *transformed into heat*, by which, at the places where the crushing sufficiently takes place, the material of the rock so crushed and of that adjacent to it are heated even to fusion. The access of water to such points determines volcanic eruption. Volcanic heat, therefore, is one result of the secular cooling of a terraqueous globe subject to gravitation, and needs no strange or gratuitous hypothesis as to its origin."

It is readily seen how important a bearing these conclusions have upon the question of the moon's condition. So far, at any rate, as the processes of contraction and the consequent crushing and dislocation of the crust are concerned, we see at once that in the case of the moon these processes would take place far more actively than in the earth's case. For the cooling of the moon must have taken place far more rapidly, and the excess of the contraction of the nucleus over that of the crust must have been considerably greater. Moreover, although the force of gravity is much less on the moon than on our earth, and therefore the heat developed by any process of contraction correspondingly reduced, yet, on the one hand, this would probably be more than compensated by the greater activity of the lunar contraction (*i. e.*, by the more rapid reduction of the moon's heat), and on the other, the resistance to be encountered in the formation of elevations by this process would be reduced

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portant series of experiments completed by him :—the one on the actual amount of heat capable of being developed by the crushing of sixteen different species of rocks, chosen so as to be representative of the whole series of known rock-formations from oolites down to the hardest crystalline rocks ; the other, on the coefficients of total contraction between fusion and solidification, at existing mean temperature of the atmosphere, of basic and acid slags analogous to melted rocks. The latter experiments were conducted on a very large scale ; and the author points out the great errors of preceding experimenters, Bischoff and others, as to these coefficients. By the aid of these experimental data, he is enabled to test the theory produced when compared with such facts as we possess as to the rate of present cooling of our globe, and the total annual amount of volcanic action taking place upon its surface and within its crust. He shows, by estimates which allow an ample margin to the best data we possess as to the total annual vulcanicity, of all sorts, of our globe at present, that less than one-fourth of the total heat at present annually lost by our globe is upon his theory sufficient to account for it ; so that the secular cooling, small as it is, now going on, is a sufficient *primum mobile*, leaving the greater portion still to be dissipated by radiation. The author then brings his views into contact with known facts of vulcanology and seismology, showing their accordance. He also shows that to the heat developed by partial tangential thrusts within the solid crust are due those perturbations of hypogeal increment of temperature which Hopkins has shown cannot be referred to a cooling nucleus and to differences of conductivity alone."

precisely in the same proportion that gravity is less at the moon's surface. It is important to notice that, as Mr. Mallet himself points out, his view of the origin of volcanic heat "is independent of any particular thickness being assigned to the earth's solid crust, or to whether there is at present a liquid fused nucleus,—all that is necessary being a *hotter* nucleus than crust, so that the rate of contraction is greater for the former than for the latter." Moreover, "as the play of tangential pressures has elevated the mountain-chains in past epochs, the nature of the forces employed sets a limit" to the possible height of mountains on our globe. This brings Mr. Mallet's views into connection with "vulcanicity produced in like manner in other planets, or in our own satellite, and supplies an adequate solution of the singular, and so far unexplained, fact, that the elevations upon our moon's surface and the evidences of former volcanic activity are upon a scale so vast when compared with those upon our globe."

All that seems wanted to make the explanation of the general condition of the moon's surface complete, according to this theory, is the presence of water in former ages, over a large extent of the moon's surface,—*unless* we combine with the theory of contraction the further supposition that the downfall of large masses on the moon produced that local fusion which is necessary to account for the crateriform surface-contour. It is impossible to contemplate the great mountain-ranges of the moon (as, for instance, the Apennines under favourable circumstances of illumination), without seeing that Mallet's theory accords perfectly with their peculiar corrugated aspect (the same aspect, doubtless, which terrestrial mountain-ranges would exhibit if they could be viewed as a whole from any suitable station). Again, the aspect of the regions surrounding the great lunar craters—and especially the well-studied crater Copernicus—accords closely, when sufficient telescopic power is employed, with the theory that there has been a general contraction of the outer crust of the moon, resulting in foldings and cross-foldings, wrinkles, corrugations, and nodules. But the multiplicity of smaller craters does not seem to be explained at all satisfactorily; while the present absence of water, as well as the want of any positive or direct evidence that water ever existed upon the moon, compels us to regard even the general condition of the moon's surface as a problem which has still to be explained. If, however, it be admitted that the processes of contraction proceeded with sufficient activity to produce fusion in the central part of a great

region of contracting crust, and that the heat under the crust sufficed for the vaporisation of a considerable portion of the underlying parts of the moon's substance, we might find an explanation of the great craters like Copernicus, as caused by true volcanic action. The masses of vapour which, according to that view, sought an outlet at craters like Copernicus must have been enormous however. Almost immediately after their escape they would be liquefied, and flow down outside the raised mouth of the crater. According to this view we should see, in the floor of the crater, the surface of what had formerly been the glowing nucleus of the moon: the masses near the centre of the floor (in so many cases) might be regarded as, in some instances, the *débris* left after the great outburst, and in others as the signs of a fresh outburst proceeding from a yet lower level; while the glistening matter which lies all round many of the monster craters would be regarded as the matter which had been poured out during the outburst.

We need not discuss in this connection the minor phenomena of the moon's surface. It seems evident that the *rilles*, and all forms of *faults* observable on the moon's surface, might be expected to result from such processes of contraction as Mallet's theory deals with.

It is, in fact, the striking features of the moon's disc—those which are seen when she is examined with comparatively low telescopic powers—which seem to tax most severely every theory which has yet been presented. The clustering craters, which were compared by Galileo to "eyes upon the peacock's tail," remain unaccounted for hitherto; and so do the great dark regions called seas. Mallet's theory explains, perhaps, the varieties of level observed in the moon's surface-contour, but the varieties of tint and colour remain seemingly inexplicable.

There is one feature of the lunar globe which presents itself to us under a wholly changed aspect if we adopt Mallet's theory. I refer to the radiations from certain great craters, and especially those from Tycho, Copernicus, Kepler, and Aristarchus. The reader is doubtless aware that an attempt has been made to explain these radiations by comparing them to the fissures produced when hollow globes are burst by pressure from within. It is in this way that Mr. Nasmyth accounts for these striking features of the moon's disc. But it has been objected that if such fissures were formed and filled up by matter extruded from the interior of the satellite, it could not but happen that along some portions of the length of each fissure the original

contour of the surface would not be restored,—either an excess of matter being forced up through the opening or a part of the opening left unfilled,—and that the resulting inequalities could not fail to be rendered discernible under oblique illumination. According to any theory which accounted for these features as due to internal forces acting outwards, it was exceedingly difficult to interpret the fact that along the whole length of these rays there can be observed a peculiar difference of brightness under direct illumination, while, nevertheless, such features of the surface as craters, mountain-ranges, plains, and so on, extend unbroken over the rays. I do not know that the theory of contraction serves to meet the difficulty completely; in fact, the difference of tint in the rays and the circumstance that the rays can only be well seen under full illumination appear to me to be among the most perplexing of the many perplexing phenomena presented by the moon's surface. But so far as the mere formation of radiations of enormous length is concerned, it seems to me that we have a far more promising interpretation in the theory of contraction than in any theory depending on the action of sublunarian forces. For whenever an outer crust is forced to contract upon an enclosed nucleus, a tendency can be recognised to the formation of radially arranged corrugations. Nevertheless, it may be questioned whether—when this tendency is most clearly recognised—there is not always present some unyielding matter which forms a centre round which the radiations are formed; and it is somewhat difficult to see how or why such centres of resistance should exist in the case of the lunar crust. It is a little remarkable that here again we find ourselves led to entertain the notion that matter arriving from without has produced these sublunarian *knots*, if one may so speak, whose presence is not directly discernible, but is nevertheless strikingly indicated by these series of radiating streaks.

The circumstance already referred to, that these rays can only be well seen when the moon is full, has long and justly been regarded as among the most mysterious facts known respecting the moon. It is difficult to understand how the peculiarity is to be explained as due merely to a difference of surface-contour in the streaks; for it is as perplexing to understand how the neighbouring regions could darken from this cause just before full moon, and remain relatively dark during two or three days, as to explain the peculiarity by supposing that the rays themselves grow relatively bright. It is true that there are certain surfaces which

appear less bright under a full than under an oblique illumination,—using the words "full" and "oblique" with reference to the general level of the surface. But the radiations occupy arcs of such enormous length upon the moon's surface, that the actual illumination of different parts of the radiations varies greatly, and of course there is a like variation in the illumination of different parts of the regions adjacent.

It is natural, under these circumstances, to inquire how far it is probable (1) that real processes of change take place month by month on the moon's surface, and (2) that it is to these processes that we owe the greater or lesser distinctness with which certain features present themselves.

It is known that Dr. De la Rue was led, by his photographic researches into the moon's condition (for we may fairly thus describe his experience in lunar photography), to the conclusion that processes resembling vegetation take place on the moon, the period during which the vegetation passes through its series of changes being a lunar month. He was particularly struck by the circumstance that portions of the moon which seem equally bright optically are by no means equally bright chemically. "Hence," he says, "the light and shade in the photograph do not correspond with the light and shade in the picture; and therefore the photograph frequently renders visible details which escape optically. Those portions of the moon near the dark limb are copied photographically with great difficulty, and it frequently requires an exposure five or six times as long to bring out those portions illumined by a very oblique ray, as others apparently not more bright when more favourably illuminated. The high ground in the neighbourhood of the southern portion of the moon is more easily copied than the low ground, usually called seas, and I have ventured to suggest that the moon may have an atmosphere of great density, but of small extent; and this idea has, I imagine, received some confirmation from a recent observation of Father Secchi's, of the lunar surface polarising light more in the great lowlands and in the bottoms of the craters, and not appreciably on the summits of the mountain-ridges."

It is extremely important to notice that photography shows the light near the terminator to be less bright than it appears to the eye. It may be, of course, that the distinction resides mainly or entirely between the photographic power and the luminosity of these portions; there may, for example, be an excess of yellow light and a deficiency of

green, while the greater photographic power of the parts under full solar illumination may indicate an increase of green light due to some process of vegetation. It is, however, important to inquire whether the greater part of the difference may not be due to a physiological cause; whether, in fact, the neighbourhood of the dark portion of the disc may not cause the illuminated parts near the terminator to appear, through contrast, brighter than they really are.

On the answer which may be given to this question depends, in a great degree (as it seems to me), the opinion we are to form of those recent researches by Mr. Birt which have appeared to indicate that the floor of Plato grows darker as the sun rises higher above it. Taking these researches in their general aspect, it cannot but be recognised that it is a matter of the utmost importance to determine whether they indicate a real change or one which is only apparent. If it is really the case that Plato grows darker under a rising sun, we should have to infer that in the case of Plato certainly, and probably in the case of other regions similarly placed, processes of change take place in each lunation which correspond (fairly) with what might be expected if these regions became covered with some sort of vegetation as the lunar month (or, which is the same thing, the lunar day) proceeds. Other explanations—meteorological, chemical, or mechanical—might indeed be available, yet in any case conclusions of the utmost interest would present themselves for consideration.

It must be remembered, however, that thus far Mr. Birt's observations (as well those made by himself as those which he has collected together) are based on eye-estimations. Nothing has yet been done to apply any photometric test to the matter; nor has the floor of Plato been brought alone under observation, but other light, of varying degrees of intensity, has always been in the field of view. Plato is seen bright when near the "terminator," and growing gradually darker as the sun rises higher and higher above the level of the floor of the crater. The point to be decided is, how far the brightness of Plato near the terminator is an effect of contrast. De la Rue's photographic observations go far to prove (they at least strongly suggest) that contrast has much to do with the matter. He has shown that, photographically, the parts near the terminator are not so bright as they look. May it not be that they look brighter than they are in reality? We have only to suppose that De la Rue's photographic results represent pretty accurately the true relative luminosity of different parts of the moon to answer this question at once in the affirmative.

It seems to accord with this view, that the greater darkness of the floor of Plato agrees, according to Mr. Birt's light curves, with the time when the sun attains his greatest elevation above the level of the floor. For if the action of the sun were the cause of the darkening we should expect the greatest effect to appear some considerable time after the sun had culminated (as supposed to be seen from the floor of Plato). We know that on our own earth all diurnal solar effects, except those which may be described as optical, attain their maximum after the sun has reached his highest point on the heavens, while all annual solar effects attain their maximum after midsummer. If an observer on Venus could watch the forests of our north temperate zones as they became clothed with vegetation and were afterwards disrobed of their leafy garment during the progress of the year, it would not be on the 21st of June that he would recognise the most abundant signs of vegetation. In July and August vegetation most richly clothes the northern lands of our earth. It is then also that the heat is greatest; *that* is the time of true midsummer as distinguished from astronomical midsummer. And in like manner the true heat-noon is at about two o'clock in the afternoon, not at the epoch when the sun is highest, or at astronomical noon. The difference in either case amounts to about one-twelfth part of the complete period in question: in one case we find the maximum of heat a month or twelfth part of the year after the time of the sun's greatest northerly declination; in the other we find the time of greatest heat two hours or one-twelfth part of a day after the time of the sun's greatest elevation. If we take a corresponding portion of the lunar month, we find that the greatest effect of any solar action on the floor of Plato might be expected to take place about two-and-a-half days after the sun had attained his greatest elevation. This differs to a sufficient degree from Mr. Birt's estimate to justify the suspicion that either the effect is physiological, or that it is purely an optical peculiarity, that is, due to the manner in which the light falls on a surface of peculiar configuration.

It does not appear to me, I may remark further, that Mr. Birt has *demonstrated* the occurrence of real variations in the condition of the spots upon the floor of Plato. He has ascertained that some of these are at times relatively darker or brighter than at others, and that this is not a mere physiological effect is proved by the fact that the result has been obtained by comparing the spots *inter se*. Nevertheless it must not be forgotten how largely the presentation of the

floor of Plato towards the terrestrial observer is affected by libration, now tilting the floor more fully towards the observer and presently tilting it away from him; at one time tilting the floor eastwards, at another westwards, and at intermediate periods giving every intermediate variety of tilt, —these changes, moreover, having their maximum in turn at all epochs of the lunation. Combining this consideration with the circumstance that very slight variations in the presentation of a flattish surface will cause certain portions to appear relatively dark or relatively light, it appears to me that a case has not yet been made out for those selenographical changes by which Mr. Birt has proposed to interpret these phenomena.

Nevertheless it cannot be insisted on too strongly that it is from the detailed examination of the moon's surface that we can now alone hope for exact information as to its present condition and past history. I would even urge, indeed, that the detailed examination at present being carried out is not sufficiently exact in method. I should be glad to hear of such processes of examination as were applied by Mr. Dawes to the solar spots. In particular it seems to me most important that the physiological effects which render ordinary telescopic observation and ordinary eye-estimates of size, brightness, and colour deceptive, should be as far as possible eliminated. This might be done by so arranging the observations that the conditions under which each part of the moon should be studied might be as far as possible equalised during the whole progress of the lunation. Thus, returning to the case of the floor of Plato: this region should not be examined when Plato is near the terminator as well as at the time of full moon, with the rest of the moon's disc or large portions thereof in the field of view; the eye of the observer should be protected from all light save that which comes from the floor itself; and, moreover, the artificial darkness produced for this purpose should be so obtained that the general light of the full moonlight should be excluded as well as the direct light from the disc. Then differences of tint should be carefully estimated either by means of graduated darkening-glasses, or by the introduction of artificially illuminated surfaces into the field of view for direct comparison with the lunar region whose brightness is to be determined.

When observations thus carefully conducted are made, and when the effects of libration as well as of the sun's altitude above the lunar regions studied are carefully taken into account, we should be better able than we are at present, as

it appears to me, to determine whether the moon's surface is still undergoing changes of configuration. I cannot but think that such an inquiry would be made under more promising circumstances than those imagine who consider that the moon's surface has reached its ultimate condition, and that therefore the search for signs of change is a hopeless one. So far am I from considering it unlikely that the moon's surface is still undergoing change, that, on the contrary, it appears to me certain that the face of the moon must be undergoing changes of a somewhat remarkable nature, though not producing any results which are readily discerned by our imperfect telescopic means. It is not difficult to show reasons at least for believing that the face of the moon must be changing more rapidly than that of our earth. On the earth, indeed, we have active subterranean forces which may, perhaps, be wanting in the moon. On the earth, again, we have a sea acting constantly upon the shore—here removing great masses, there using the *débris* to beat down other parts of the coast, and by the mere effect of accumulated land-spoils acquiring power for fresh inroads. We have, moreover, wind and rain, river action and glacier action, and, lastly, the work of living creatures by land and by sea; while most of these causes of change may be regarded as probably, and some as certainly, wanting in the case of our satellite. Nevertheless there are processes at work out yonder which must be as active, one cannot but believe, as any of those which affect our earth. In each lunation the moon's surface undergoes changes of temperature which should suffice to disintegrate large portions of her surface, and with time to crumble her loftiest mountains into shapeless heaps. In the long lunar night days of fourteen hours a cold far exceeding the intensest ever produced in terrestrial experiments must exist over the whole of the unilluminated hemisphere; and under the influence of this cold all the substances composing the moon's crust must shrink to their least dimensions,—not all equally (in this we find a circumstance increasing the energy of the disintegrating forces), but each according to the quality which our physicists denominate the coefficient of expansion. Then comes on the long lunar day, at first dissipating the intense cold, then gradually raising the substance of the lunar crust to a higher and higher degree of heat until (if the inferences of our most skilful physicists and the evidence obtained from our most powerful means of experiment can be trusted) the surface of the moon burns (one may almost say) with a heat of some 500° F. Under this tremendous heat

all the substances which had shrunk to their least dimensions must expand according to their various degrees,—not greatly, indeed, so far as any small quantity of matter is affected, but to an important amount when large areas of the moon's surface are considered. Remembering the effects which take place on our earth, in the mere change from the frost of winter to the moderate warmth of early spring, it is difficult to conceive that such remarkable contraction and expansion can take place in a surface presumably less coherent than the relatively moist and plastic substances comprising the terrestrial crust, without gradually effecting the demolition of the steeper lunar elevations. When we consider, further, that these processes are repeated not year by year, but month by month, and that all the circumstances attending them are calculated to render them most effective because so slow, steadfast, and uniform in their progression, it certainly does not seem wonderful that our telescopists should from time to time recognise signs of change in the moon's face. So far from rejecting these as incredible, we should consider the wonder rather to be that they are not more commonly seen and more striking in their nature. Assuredly there is nothing which should lead our telescopists to turn from the study of the moon, as though it were hopeless to seek for signs of change on a surface so desolate. Rather they should increase the care with which they pursue their observations, holding confidently the assurance that there are signs of change to be detected, and that in all probability the recognition of such change may throw an instructive light on the moon's present condition, past history, and probable future.

IV. A SOLUTION OF THE SEWAGE PROBLEM.

THE treatment of sewage has long been an important question of Sanitary Reform. But the discussion involves a second question equally momentous,—that is, its utilisation, which is by no means implied in mere deodorisation or disinfection. A forcible illustration of this is found in Lord Palmerston's celebrated definition—"Dirt is matter in the wrong place." The offensive elements contained in sewage are in the wrong place when sent in to the river, but are in their right place when they are separated from it, and reserved, like the farmer's manure

heap, for restoration to the land at the proper time. We invite epidemics if we permit the former; and we must cease to expect a fair supply of corn, wine, and oil, or the other bounties of Nature, if we neglect the latter, while we continue to draw from the land all its nutritive properties. The value of land is daily increasing, and therefore the highest possible cultivation becomes necessary. The only means of increasing its productive powers is by manuring, and for this purpose *all* matters possessing real fertilising value becomes a point of the first importance.

Many methods for dealing with the sewage of towns have been proposed. They may be classed under the four following schemes:—

1. Irrigation.
2. Filtration.
3. Destruction.
4. Precipitation.

These schemes may be considered individually or collectively in certain combinations.

Let us deal first with irrigation, and we may say at once that with us it has no favour, for it has been abundantly proved that at the best it is a disposal of sewage merely, and in no way its utilisation; for the excessively rank vegetation of a sewage farm forced to take more than is good is no more an evidence of high farming than was Wackford Squeers an evidence of the high feeding of the Yorkshire school. But even as a *disposal* of sewage it falls lamentably short of efficiency, as may be seen by any impartial inquirer. Under the most favourable circumstances this system is inadequate to deal with the entire sewage; for the quantity of land required annually to deodorise this (one acre for 100 people) is so large, in proportion to the land available for the purpose, that for financial, geological, and local reasons, the system could not succeed. There are other objections to irrigation with fluid sewage. Land for the purpose must be in propinquity to the town to which the system is applied, and this land may have to be bought in by the pressure of an Act of Parliament, at great expense, as it is generally opposed by wealthy landowners. Such opposition is to be expected; for the neighbourhood of a sewage farm would certainly not be selected by the rich as a site for their mansions; and the value of land is consequently deteriorated.

The charge of miasmatic emanations arising from a system of sewage irrigation has been abundantly proved by evidence given before the House of Commons by eminent medical and sanitary experts:—

Mr. Thomas Hawkesley, C.E., says (in reference to the Blackburn Corporation Improvement Bill, March 15th, 1870)—"Water irrigation carried on in warm weather is exceedingly unhealthy; in fact you make, so to speak, a kind of fen of the large area of land which you put the water over." . . . "Where the water is foul I can speak positively to it, from repeated observation in different places, that the odour, particularly at night, and particularly upon still damp evenings in autumn, is very sickly indeed, and that in all these cases a great deal of disease prevails; but I need not do more than upon that subject refer to the evidence taken by the General Board of Health itself." . . . "With regard to sewage irrigation this happens:—The sewage forms a deposit on the surface of the ground; that deposit forms a cake of organic matter; and that organic matter, when it is in a damp state, as it usually is, gives off in warm weather a most odious stench." Of the Barking farm Mr. Hawkesley says—"The stench was of a very fœtid character indeed, and of very considerable intensity." At Edinburgh, at Carlisle, and at Harrogate, the state of the atmosphere varies with the state of the weather. Of Edinburgh the witness says—"I cannot call it a mere odour in the ordinary sense. Everybody who walks down to Leith from Edinburgh, or to Portobello, in warm weather, cannot help being assaulted by it." At Carlisle, "they were utilising only about one-sixth of the sewage." At Croydon, where the soil is the most favourable that could be had, consisting of only a slight covering of alluvial matter upon chalk, gravel, and gravel-flints, "the people complain of this fœtid smell in summer, and particularly at night, and of a very low state of health in consequence;" and "the water does not run off clear," "nor nearly free from organic matter." At Birmingham, "It has a very prejudicial influence on the value of property." "Irrigation works with sewage water for the utilisation of sewage are most pernicious." Mr. W. E. Cressy, M.R.C.S., states, to the same Committee, that in the case of the sewage farm belonging to the Croydon Board of Works there has been, since 1867, typhoid fever in every cottage on the estate, which he refers to the existence of the farm. The water from the wells in the neighbourhood becomes putrid if allowed to stand for 24 hours. Cows feeding on the grass from this land yield milk which has been proved, by a series of experiments, to cause fever.

Dr. Henry Letheby, Medical Officer of Health to the City of London, gave evidence before the House of Commons in reference to both the Blackburn and Reading

Bills, on the 15th and 25th of March, 1870. He states that, taking the condition of the sewage put upon the land at Croydon, Norwood, Beddington, Rugby, Carlisle, and Worthing, the average proportions of matter in solution in the sewage, before it was put upon the land, was 32·77 grains. As it ran from the land it contained 34·3 grains, there being an increase in the solid matter after flowing through the land. The necessary conditions for irrigation, which he admits are not always present, are porous soil and good subsoil drainage. Frozen soil will not allow the sewage to sink, and a heavy rainfall will prevent it; and Dr. Letheby's experience has shown him that the land acts upon the sewage only at the time of active vegetation, "but that during the time of the dormant state of the vegetation the sewage runs off that land pretty nearly as it goes on it." He shows that, besides the acre of land for every 100 people, there must be another acre in reserve when that cannot be doing its work. The chief objections he considers to be, in the first place, the saturation of the soil with excrementitious matter, which is constantly giving off—sometimes to a great extent, at other times not so much—effluvia capable of producing disease. Secondly, "the subsoil water is always charged with decomposing matters, the residue of the sewage; and we know from the investigations recently of Dr. Pettinkoffer, who has examined into the question in England and Germany, and almost all over the world, that there is no more fruitful source of disease than a subsoil water charged with offensive matters, and altering in its level. The soil becomes filled with offensive gases, and he traces cholera and typhoid fever to these emanations, and he attributes epidemics to these emanations. Again, we have subsoil water which runs into the neighbouring wells, and whenever there is subsoil irrigation the neighbouring wells are offensive." . . . "There is another objection, which I look upon as the most serious of all: parasitic diseases in the human body are always derived from parasitic diseases in the flesh of the animals we eat. I hold in my hand a report from the most experienced man in this subject,—I may say in the world,—Dr. Cobbold. It treats of the more than probable, the certain, introduction of serious parasitic disease among the community, if sewage be put upon land as a means of utilising it."

These are the objections to the utility of the process of irrigation merely as a means of *disposal* of the sewage,—and they are very great,—whilst as we before observed, as to the equally important question of utilisation, its claims are very small indeed.

The abundance of crops produced on a given area has been quoted in favour of the system of irrigation. The finest manurial substances are possessed by the constituents of sewage; but the irrigationist is so wasteful in their application, that, in the majority of cases, there ensues not a healthy crop, but a mass of overgrown, rank grass material, of no more nutritive value than weeds; for be it distinctly remembered that this is not a question of manuring with sewage when necessary,—but the compulsory application of enormous quantities, in season and out of season, till the surfeited land is sick, and even then it has to take more still. If this waste were prevented, by the conversion of the sewage into a dry, portable, inoffensive manure, then this manure might be stored until it could be employed at the proper season without injurious effect; but to dose vegetation with equal quantities of manure, day by day throughout the year, is an absurdity which of itself is sufficient to condemn sewage irrigation.

The second process, that of filtration, appears to be involved in some obscurity,—that is to say, there are attached to the term several meanings, of greater or less comprehension. Not a little of the confusion appears due to the Rivers' Pollution Commission having discussed "intermittent downward filtration," without defining the term. We are told that irrigation owes no inconsiderable amount of its success to the contemporaneous effect of filtration of sewage through the soil, and, confusion worse confounded, we are instructed that "irrigation involves filtration." We, however, will take filtration to mean the passing of the sewage water through an artificially-constructed bed of sand, charcoal, &c. Filtration by itself is simply a method of disposing of sewage, not of utilising it, and therefore we hold it in no more favour than the other; for we maintain that unless the manurial elements are preserved *for* the land, as well as *from* the river, the problem is but half solved. Filtration processes do not profess to do the former, and as for the latter we do not find that they are very successful, so far as efficiency is combined with economy.

Let us revert, for an instant only, to the filtration—intermittent, or downward, or irrigation-filtration, or otherwise—of the Rivers' Commission. We will first describe the construction of such a filtering-bed, and will then take in consideration of the efficacy of this quasi-filtration the evidence of Dr. Frankland. The illustration is the construction of the Merthyr Tydvil beds, described by Mr. T. C. Scott, a strenuous advocate. "The filtering

medium consists of 20 acres of land, drained 6 feet deep, and divided into four areas of 5 acres each. Each of these receives daily, for six hours out of the twenty-four, the sewage of 20,000 persons, represented by 900,000 gallons, or at the rate of 73,000 tons per acre per annum. To utilise advantageously, according to our present knowledge, the quantity of sewage thus dealt with, 200 acres of land are required, being at the rate of 1 acre for every 100 persons, or for 7,300 tons of this sewage." Now for the opinion of Dr. Frankland, who is an advocate of the irrigation system, and a Rivers' Pollution Commissioner. "I think it (this downward filtration) is an important part of our knowledge; but although I have had so much to do with it, I confess I am not very sanguine of its success as applied to large volumes of sewage, and for this reason: you collect upon the surface of your filters a large quantity of suspended matter from the sewage, which is fœcal matter in a state of decomposition, and we should be afraid that this matter so collected would be offensive to the neighbourhood. No plant can live upon the filter which is deluged in this way with sewage. This cannot be carried out along with plant growth, and consequently you have not the removal of those noxious constituents which accumulate on the surface by plant life, such as you have in irrigation." Thus, the filtration of unprepared sewage leaves us with far higher chances of miasma than do the evils of irrigation.

The process known as Weare's is a true filtration process, and is on a small scale said to be satisfactory. It has been employed in the workhouse at Stoke-upon-Trent; the filtration being effected through vegetable charcoal and fine ash, altogether a different method to the irrigation-filtration system. The filtering medium is placed in tanks through which the sewage percolates. The effluent water, however, still contains in solution a large proportion of putrescible organic matter, and is below the standard required by the Rivers' Pollution Commissioners, or by the Conservancy of the Thames.

But the evidence brought before the Parliamentary Committee on the Birmingham Sewerage Bill, in April and May last, has, we think, given the death-blow to sewage filtration. After fourteen days' hearing of the evidence of the leading authorities in Chemistry, Engineering, and Agriculture, the Select Committee attached to their approval of the Bill the condition that "*No sewage be put upon any land without having been previously defecated in tanks.*"

The third scheme of getting rid of sewage, viz., that of

destruction, requires only a brief notice. By its very nature it forces us to condemn it: destruction has always been a favourite method of disposal of inconvenient elements, from time immemorial. Considerable difference of opinion exists as to what constitutes inconvenient elements,—but once admit the principle, and we find men using it to justify the murder of children by Lycurgus, and the monster fires of religious persecution. For our own part nothing will satisfy us but rational utilisation. Under the head of destructive processes we include the lime process of Tottenham and General Scott's cement process. By General Scott's process an effluent of a low standard of purity is obtained, whilst the result is in an agricultural point of view the most wasteful that could be devised. The sludge instead of being returned to the land is employed in the making of a kind of Portland cement. Human beings live directly and indirectly upon the produce of the land. Now scientific agriculture tells us that land unless properly manured becomes soon exhausted; and it is clear that the waste products of human beings, being most valuable in rendering the land fertile, should be returned to the land as manure, and not be destroyed. Few persons have any idea of the enormous waste which is committed in casting London sewage into the Thames. Mr. Mechi, a great authority on all agricultural subjects, tells us that the inhabitants of London consume *daily* the annual available produce of 20,000 acres, and a similar quantity is required weekly for London horses. The manurial wealth of this 20,000 acres of land is absolutely wasted, and the country thereby loses as much food as if three million quartern loaves were daily floating down the Thames towards the sea.

We come now to the consideration of the fourth scheme of defœcating sewage, by precipitating the solid constituents. The object of precipitation is to remove in a solid, dry, or semi-dry state the putrescible constituents of the sewage, and to render the filtrate or effluent water sufficiently pure to mingle with our streams, or be employed for purposes of irrigation. There are several processes which profess to remove more or less of the impurities from the water. Amongst these may be mentioned the Phosphate Sewage Company's process, the lime precipitation process, and the A B C process. In the Phosphate Sewage Company's process "the water is left still maintaining all its nitrogenous and valuable properties, *plus* any excess of phosphoric acid which has been added, and, therefore, highly useful for the irrigation of cereals and other crops." This process, which

was invented by David Forbes, F.R.S., when compared with those with which we have been dealing, appears a very admirable one. It is, however, from a technological point of view, somewhat deficient in economy. For the ingredient added to the sewage is expensive, too expensive with regard to the return in practical good obtained from its use in agriculture; and unfortunately those plants which require most phosphoric acid bear irrigation least. In a theoretical point of view, if we overlook the infringement of the rule of economy, the process is attended with a high degree of consistency. By no means is the preceding statement true of all precipitation processes. The lime process, as an instance, produces a precipitate containing a large proportion of lime, possessing but feeble or no manurial power, and readily putrefying; while the effluent water, instead of being pure or even suited to the purposes of irrigation, contains introduced foreign matter inimical to the land and the life of plants.

The process of precipitating by sulphate of alumina the valuable constituents of sewage, and utilising at the same time the purifying power of charcoal and clay, is that to which we decidedly give the preference, as by this means the water is practically purified fit to be discharged into a running stream, and the deposit is retained in a form entirely inoffensive and capable of being turned into a dry and portable manure. This process has been before the world for some years as the A B C process, worked by a company called the Native Guano Company, and the claims it set up three years ago to have solved the great social problem we may now pronounce to be fully justified by facts; its principles were correct, the mechanical arrangements for conducting them being alone defective. The name of the process has been derived from the initial letters of the principal constituents of the precipitant: Alum, Blood, Clay, and Charcoal. We will consider the action of these substances upon sewage, taking them in order.

The alum was for a considerable time a source of expense, it being added to the sewage in the form of ammonia-alum. Ammonia-alum has the further disadvantage that the ammonia remains in the effluent water. A much more economical, and as effective a substitute has been found in a crude sulphate of alumina manufactured at one-fourth the cost.

The action of the sulphate of alumina may be briefly described.

In contact with sewage,—a slightly alkaline liquid charged with nitrogenous organic matter,—the alumina is

separated in flocks, and, by virtue of its remarkable affinity for dissolved organic matter, each particle seizes hold of, and drags down with it, a corresponding particle of nitrogenous impurity. The blood here comes into play; this is essentially a liquid highly charged with albumen; albumen is instantly coagulated in the presence of alum; and in the same way as this ready coagulability of albumen is utilised in fining wine and coffee, so it is made use of in this process by joining with the alumina in its precipitation, uniting it in a net-work of fibres, and giving it, as it were, arms wherewith to seize upon and drag out of solution still more putrescible constituents.

But the precipitated hydrate of alumina is light in character, and although it would ultimately settle, leaving a clear liquid above it, the slightest agitation causes it to float up, and thus renders it difficult, on the large scale, to drain off the mud. At Paris sulphate of alumina has lately been employed for clarifying several hundred thousand gallons of sewage; and among the many defects of this process, that of imperfect settlement was by no means the least. Here the action of the clay is apparent. This substance has a curious physical property; when finely ground up with water it forms a creamy emulsion, which takes many days to settle; many rivers, in time of flood, owe their turbidity to this cause: the Seine at the present time is a striking example, its water being in colour, although not in actual impurity, as bad as the Thames below London Bridge. But when this creamy liquid meets with sulphate of alumina, the clay coagulates like albumen, and settles down in heavy granular flakes. Now in the A B C process these three precipitations—that of the alumina, that of the albumen, and that of the clay—take place simultaneously, and in each other's presence; they become closely locked together in a triple alliance; the heavy character of the clay particles gives density to the mass, and causes it to settle rapidly, and remain in a compact form at the bottom of the tank.

Were the object merely to produce an easily dried precipitate and a clear effluent, nothing more would be required; for not only has this precipitate carried down all the suspended matter, but much of the dissolved nitrogenous and albumenoid impurities have fixed themselves on to the alumina, whilst the clay has also performed its part in absorbing and carrying down a good proportion of the ammonia. But there still remains the probability, if not the certainty, of foul gases being present, whilst the water, though clear,

may nevertheless be coloured. These residual impurities are attacked by the charcoal: the powerful affinity of animal charcoal for organic colouring matter corrects the one evil, whilst the well-known absorptive action exerted by vegetable charcoal on the gaseous products of putrefaction corrects the other. In the way of purification little more remains to be done.

These reactions, by a modification in the order in which the purifying ingredients are added, are effected at once, with a certainty of uniform results, and, by a simple mechanical arrangement, variation in the dose of each constituent required by a variation in the strength of the sewage can be readily controlled.

The method of applying the ingredients is extremely simple. The clay and charcoal are incorporated in a grinding mill with the aid of sufficient water to form a thin paste. This paste flows into a tank, and is constantly agitated until it is required to be mixed with the sewage. By the side of the mixing-room is a smaller room, through which passes a channel or trough. At one end of this channel there rushes in the London sewage, and with it an unmistakable odour. The B C mixture or thin water-paste of clay and charcoal is admitted to the trough by a pipe from the store-tank; the sewage in its passage past this pipe carries with it the mixture, and the two after well mixing proceed on their way past a second pipe connected with a tank containing a supply of sulphate of alumina dissolved in water. All that is now requisite is to allow the sewage, B C mixture, and alum to flow in inodorous company to the settling tanks. The channel leading to the tanks has its course interrupted by numerous ledges, which serve to cause the more perfect intermixture of the sewage and the disinfectants. The first tank in which the sludge is allowed to settle contains the principal portion of the precipitate. The clear water is allowed to flow off continuously from the first tank into a second tank; and the remainder of the mud is deposited in this and in the other tanks into which it flows. From the last tank the water is conducted to the river, appearing as a clear, inodorous, and tasteless effluent. When sufficient sludge has been collected in the first tank, the treated sewage is shut off from this, and permitted to flow into another tank, which then forms the first of the series. As much of the water as possible is then run off from the mud, and the latter is drawn into the acidifying tanks, where a small quantity of sulphuric acid is added to prevent the loss of any ammonia. From

the acidifying tanks the semi-dry mud is pumped into the drying-presses, whence it issues in a cake. This semi-solid mud is then further dried by a most ingenious application of heat in revolving iron cylinders. The wet mud is passed in at one end, and dry manure, in the form of an inodorous and inoffensive powder, falls from the other end, at the rate of 5 tons in ten hours, at an expenditure of a few cwts. of coal.

If space enough be available the mud may be simply pumped from the bottom of the settling tanks into large open-air stanks, where it dries under the influence of the sun and air. Not the slightest offensive odour is apparent during any stage of this drying.

The dry mud in powder, and forming excellent manure, is removed from the sheds, and packed into bags for transport.

We have thus traced the process from the sewage to the manure and the effluent water. Before entering upon any statements with regard to the value of the results, we will more fully detail the process as it is followed at the experimental works at Crossness.

Crossness is situated on a projecting part of the southern shore of the Thames, between the Plumstead and Erith marshes, and is the southern outfall of the London drainage. The quantity of sewage now daily discharging at Crossness is 50,000,000 gallons. Large as this quantity may appear the enormous engines employed in pumping the sewage are fully equal to the task, for they are capable of lifting 280 tons in a minute, or nearly double the average flow. The transformation of such a mighty mass of filth into heaps of shining gold is a feat worthy of the days of the alchemist, or rather of the days of modern chemistry. Of this quantity of sewage the works of the Native Guano Company are capable of dealing in the twenty-four hours with 500,000 gallons, drawn from the cross-cut, or culvert through which the sewage runs into the principal reservoir. This quantity amounts to 1 per cent of the whole delivery. Thither the sewage flows into the sump of a pump worked by a 15 horse-power steam-engine, whence it flows into contact with the A B C constituents as we have described.

From the mixing trough the sewage, as described, flows to the settling tanks. These tanks are six in number, and are constructed of concrete, each being 50 feet long by 20 feet wide, and 8 feet in depth. When leaving the last settling tank the effluent water is caused to take a considerable fall,

so as to afford room for the construction of a subway in such a manner as to place the sheet of water—as clear as plate-glass—between the visitor and the diffused light of the sky. In this position the transparency of the water is subjected to a most severe test, leaving no doubt as to the previous subsidence of all solid particles. The effluent water is run off to the Thames in a shallow brick-built conduit, about 4 feet wide by 270 feet in length, and arranged during its course to form several miniature cascades.

During an official trial, lately completed, extending over eighty days, there were used 80 tons of dry A B C materials, whilst the "native guano" obtained amounted, in the dry state, to 131 tons, showing an increase of more than 63 per cent. The amount of sewage treated during this time was 11,672,000 gallons. Therefore 1 ton of dry native guano was obtained from 89,100 gallons of the Crossness sewage.

The Crossness works are calculated to have cost the Company considerably more than it would be necessary to expend upon any works dealing with much larger quantities of sewage; but it is estimated that £5000 would amply remunerate the contractors for works which should deal with the sewage of 20,000 inhabitants, and that £1000 additional capital would provide for the working expenses. This, however, is not a matter with which we have to deal in detail.

The state of the effluent water may be viewed from two points—that of an analytical chemist, and that of a practical man of the world. The former can, without difficulty, make out a case which would lead persons ignorant of the weakness of purely chemical reasoning to condemn any water in the world; and a sensation is readily created by manipulating figures in such a way as to convert *grains* of the normal constituents of a good drinking water into *tons* of impurities, and by classifying perfectly innocuous substances under the fearful title of "previous sewage contamination."

Common sense leads one to judge of a water by other standards than those of theoretical chemistry. The effluent water from sewage purified by the A B C process, falling into the Thames at Crossness, into the Aire at Leeds, into the Croal at Bolton, and into the Seine at Paris, may not at all times come up to the fanciful requirements of a scientific chemist,—although the inhabitants of many towns and villages habitually use and thrive upon a worse water—but no intelligent man of the world will doubt its suitability for admixture with ordinary river water. It is perfectly limpid and colourless; it has no smell, and so little taste that were

it not that the tasters know whence it comes they would not notice it. On standing, the water acquires no disagreeable odour; it forms no deposit, nor does it give rise to "sewage fungus" or other vegetable growth along the water-courses. Fish will live in it,—not only hardy varieties, but the more delicate kinds, such as gudgeon, to which a very slight taint of impurity is fatal. When the inquirer further finds that the effluent water is not too hard to interfere with its domestic use for washing or cooking purposes, he will endorse the opinion which the writer has deliberately formed, that there are not many English rivers on which large towns are situated which are as free from real impurity as the effluent water from sewage purified by this process.

Instead of fixing upon a fanciful standard of purity which could never be attained in practice, common sense decides that an effluent water from sewage is fit to be discharged into a running stream if it contain a less percentage of impurity than the water of that stream: the word "impurity" being not strained beyond its legitimate meaning, or made to include perfectly harmless constituents.

Let us now pass to the next point of inquiry—the manurial value of the "native guano," and the cost at which it is produced.

Of the value of a manure, chemistry can tell us little more than it can of the value of water. Just as mere chemical analysis would utterly condemn water containing Liebig's extract, infusion of tea, or a glass of bitter ale, as largely contaminated with nitrogenous organic matter or albumenoid ammonia; so chemistry, by taking a fictitious standard for manures, and judging only by the percentage of two of the many necessary constituents of the food of plants, gives an arbitrary money value to a manure, which is often exceeded by the price it fetches in the market. Agriculturists frequently pay more for nitrogenous and phosphatic manures than the price assigned to them by chemical analysis, and the sales of "native guano" form no exception to this rule.

In the autumn of last year the writer satisfied himself as to the alleged agricultural value of the manure, by personal enquiry amongst the farmers who had used it. With scarcely an exception, the farmers (of whom he saw twenty or thirty) were unanimous in their approval of "native guano:" many of them were shrewd, intelligent men, well acquainted with the various artificial manures in the market; they had tried "native guano" with intelligence on different fields against other manures, and were assured that—putting equal values

per acre—it was superior to most manures in the market. Moreover, an examination of the books of the Company shows that the good opinion of agriculturists was genuine, inasmuch as a man who, the first year, would grudgingly take 1 ton as an experiment, the next year took 10 tons, and the third year would increase his order to 20, 50, and even 100 tons, grumbling that the limited supply prevented him having all he wanted.

But it must not be imagined that the results of the laboratory and of practice are altogether anomalous in the case of the native guano manure; there is simply a difference in degree, and this difference arises from the non-existence of a fixed chemical standard of manurial worth. Nor does chemical analysis always show a low money value for "native guano." Samples submitted, at the Paris works, to one of the first analytical chemists in France (M. Terreil, Aide-Naturaliste en Chef des Travaux Chimiques au Muséum d'Histoire Naturelle) are reported by him to be worth in their dry state 108·6 francs per ton, or, when reduced by the normal amount of moisture present in "native guano," and converted into English money, £3 12s. 5d. per ton, whilst the cost of production is far below that figure.

As more particular evidence of the manurial worth of the guano, we may refer to the results obtained on the experimental farm of 7 acres established in connection with the works at Crossness. The farm, as is indeed the entire system, has lately been under the supervision of the Metropolitan Board of Works. The following are the returns from 9 yards square:—

Golden Drop Wheat.

	lbs.	ozs.
Native Guano, 15 cwts. per acre . . .	6	4½
Do. do. 10 do. do. . . .	6	1½
No Guano	2	14

White Rough Chaff Wheat.

	lbs.	ozs.
Native Guano, 15 cwts. per acre . . .	6	7
Do. do. 10 do. do. . . .	5	7
No Guano	5	4

Revet Wheat.

	lbs.	ozs.
Native Guano, 15 cwts. per acre . . .	11	0
Do. do. 10 do. do. .	10	14
No Guano.	6	8

Black Tartarian Oats.

	lbs.	ozs.
Native Guano, 15 cwts. per acre . .	6	8
No Guano	3	14

These results are worthy the attention of the farmer; but they are in no way surprising, for it is universally admitted that town sewage has manurial value; and as the ingredients of the A B C process which are added to the sewage have no *destructive* effect upon the constituents of the sewage, it would be a matter of much greater surprise if the "native guano" were found to be without manurial value. Further evidence in favour of the manure is, that there is a demand for it at the rate of £3 10s. the ton. That at Crossness the manure has cost more than this sum to produce is extremely probable, for the machinery, steam-engine, and tanks have been apparently arranged with the object of getting the minimum of work at the maximum of expense. Probably some of this is due to the necessity of erecting works before the most advantageous method of carrying on the process had been ascertained, whilst some of the apparent waste of money may be rendered necessary by the show character of the works, and the necessity of having everything aboveground to answer the accusation of improper dilution of the effluent. But when it is considered that fifteen times as much coal is being burnt there* as was sufficient for the same work at Paris; that the alum is costing three or four times as much as it need; that an experienced chemical superintendent is included among the staff; that the rest of the staff is about twice as numerous as need be; and last, though not least, that for the greater part of the three months' official trial, the sewage which has been treated has been excessively dilute, owing to heavy rains:—when all these extenuating circumstances are considered, the wonder is, not that the "native guano" produced at Crossness has exceeded £3 10s. per ton, but that the price has not risen to twice that figure. Let us turn to other works conducted on some approach to economical principles, and a very different result will be seen.

At Paris the expenses are higher than need be, owing to their being show works, and necessarily conducted with some disregard to economy. The works being simply for experimental illustration, were carried on intermittently, and were seldom in full operation, except when visitors

* This does not include coal used for artificially drying the "native guano" at Crossness.

were expected. The usual take of sewage was at the rate of 4800 gallons per hour, but on some occasions the working was pushed until the sewage was flowing at the rate of 10,000 gallons per hour. At this rate the precipitation and the settlement proceeded without difficulty, whilst the effluent continued to flow away without deterioration. Let us take the data of these works as the basis upon which to draw up a profit and loss account of a day's work.

Ten thousand gallons of sewage per hour amount to 100,000 gallons per day of 10 hours.

For this are required the following chemicals:—

	Kilos.		Kilos.	Frs.
Animal Charcoal . .	250	at 170 frs. per	1000 =	42·5
Vegetable Charcoal .	500	,, 50 ,,	,, =	25·0
Clay	600	,, 6 ,,	,, =	3·6
Lime and Blood . .	70	,, 12 ,,	,, =	0·8
Sulphate of Alumina	162	,, 130 ,,	,, =	21·0
Total used . .	1582	. . . . . .	costing	92·9

The labour consisted of—

Four men, wages per day	13·75 frs.
One superintendent	10·00 ,,
Add one extra man	4·00 ,,
	27·75 ,,

The steam-engine burnt less than half a ton of coals a week. This with a few sundries, such as oil, &c., amounted to about 36 frs. per week.

The mud was simply pumped from the bottom of the tank into an open-air stank, where it rapidly dried under the influence of the sun and wind, assisted by the porosity of the soil. The drying therefore cost nothing.

Owing to the excessive dilution of the Paris sewage from rainfall, from the copious street washings, and from the fact that most of the night-soil is carted away to La Villette, the yield of dry "native guano" was very poor, not more than 114 parts being obtained for every 100 parts of A B C materials added, as against 163 yielded under similar circumstances from London sewage. As 1582 kilos. of A B C materials were added, the "native guano" would be 1808 kilos.

The total expenses were—

Chemicals	92·90 frs.
Labour	27·75 ,,
Coal and Sundries	6·00 ,,
	126·65 ,,

As 1808 kilos. cost 126·65 frs., therefore 1000 kilos. would cost 70 frs., equal to £2 16s. per ton.

The value assigned to this manure was, as already stated, £3 12s. 5d. per ton.

Had the price been taken at which the clay alum can be made in England, viz., £2 per ton, instead of the French price, the expenses would have been still less per ton.

The writer has been allowed an opportunity of going through the accounts of the Hastings Works for the last six months. The cost of the "native guano" produced here averages £2 4s. 1d. per ton. The operations are not carried on as economically as they might be, and there are several serious items of current expense which would be avoided in subsequent works.

At Bolton, according to the certificate of the Mayor of the Corporation, who are themselves working the A B C process under a royalty, the manure is produced at a cost of £2 6s. per ton. The royalty derived by the Native Guano Company from the profits of the Corporation of Bolton amounts to 1 per cent of the entire capital invested by the Company; so that it requires but a few more applications to realise the permanent payment of a satisfactory dividend.

We are now in a position to make deductions from the evidence given before the House of Commons with regard to the value of the process.

Mr. Hawksley says:—"Now, the great virtue of this new method (A B C) is this, that while it is just as available as the old process of precipitation by lime, it produces a manure which can be sold to a profit, and the whole thing can be done in a moderate compass; and having been done in a moderate compass, of course it does not render it necessary to acquire a gentleman's estate by compulsion, or to produce these marshes which are injurious to the health of the neighbourhood. . . . The manure is now become of great value. . . . By this new process a valuable manure is produced, which sells at £3 10s. per ton, whereas the other manure (lime process) will only sell at from 1s. to 2s. 6d. per ton."

Dr. Henry Letheby says:—"The process is carried out at Leamington so satisfactorily that the effluent water is practically disinfected."

Dr. Frankland admits that he believes "the previous application of some chemical process, such as Sillar's (A B C) process, would entirely obviate that difficulty (the clogging of the filter) attending downward filtration."

There is one important property of the prepared "native

guano" which we have still to notice. During the progress of the experiments at Leeds it was discovered that the "native guano," when made into a powder and mixed with night soil, absorbed all the moisture, thoroughly deodorised it, and rendered it a dry, inoffensive, and inodorous manure, capable of being easily transported without inconvenience. So valuable was this manure found to be that it was easily disposed of at £4 per ton, in quantities of 40 tons at a time.

From this discovery it followed that the A B C mixture should be employed to precipitate the colouring matters from refuse dye-waters of large dye-works. Some experiments were instituted in the laboratory, and the results were so satisfactory that the adoption of the process would fully answer the requirements of Mr. Stansfeld's bill for preventing the pollution of rivers.

The writer has thus endeavoured to give an outline of the A B C process of utilising sewage, to state, and to answer, objections to the process. The chief objections may be summarised as follows:—That the "native guano" is of no manurial value; this statement is untrue in fact. The writer has considered this objection very fully in a letter published some months since, a portion of which may be quoted here. "When manurial value is mentioned, a distinction must be made between the value assigned by chemical analysis and by actual experiment on a farm. The former method of valuation is most erroneous, as it only takes into account two constituents, and omits others of equal necessity to the plant life. Chemical analysis would assign scarcely any or no value to such substances as sulphate of lime, soot, the warp of the Humber, and the mud of the Nile; whilst, when a chemist does assign a value in money to a guano or a superphosphate, the price he fixes has little or no relation to the actual selling price. Farmers judge of its value by actual trial on their fields. It is in this way they fix the price it is worth their while to pay for the superphosphate, and in the same manner they judge of the value of 'native guano.' My observations at Leamington and the neighbourhood proved satisfactorily to my mind that the 'native guano' made there had a very high manurial value, and the farmers to whom I spoke about it had tested it in too many ways, and were too shrewd judges of such matters to be deceived in ascribing to native guano what was really due to previous manuring." The second objection is that the cost of the manure is more than £3 10s. per ton. In some experimental cases, perhaps, the cost has exceeded this amount per ton; but in cases where actual work

has been commenced this amount has never been reached.

But let us for a moment suppose that no profit at all resulted from the sale of the manure; and that the sewage of London, we will say, had to be dealt with at the price of £2 per 100,000 gallons (and on the large scale it could certainly be treated at less than half this cost). We have then the sewage of London, amounting to 100,000,000 gallons per diem, treated (supposing the population to be 3,265,000) at 4s. per head per annum. The annual rateable property in the metropolis amounts, according to the Valuation Act of 1869, to £19,971,000. The cost of dealing with the whole of the London sewage could therefore be defrayed by a rate of 7-8ths of a penny in the pound. These facts are in themselves a sufficient recommendation of the process.

That the process should encounter opposition is not only possible but very probable. Its adoption will affect many vested interests, as well as the interests of rival schemes. But ratepayers, whether they be scientific men or not, would do well to investigate for themselves the claims of the A B C process. And not only the ratepayer, but every man who has a voice in the welfare of the nation and its production of food, or who desires that our towns should be healthy, should judge for himself of the value of the process. It may then be repeated that the claims of the A B C process to public confidence are threefold:—

I. It deodorises and disinfects sewage, and precipitates the suspended and much of the injurious dissolved matter without giving rise to any nuisance; it converts the deposit into a dry, portable, and inoffensive powder, possessing considerable manurial value.

II. It leaves the effluent water in a state of practical purity, fit to be discharged into any river.

III. It effects these important sanitary requirements at a cost, which not only relieves the ratepayers of expense, but even yields a profit, owing to the ready sale of the "native guano" at £3 10s. per ton, and its production at a cost of not more, and probably much less, than £2 a ton.

V. COLOURS AND THEIR RELATIONS.

By Mungo Ponton, F.R.S.E.

Part I.

OF all the objects of perception presented to our sight in this beautiful world, none are more generally pleasing than colours. The brilliancy of some, the delicacy of others, their varieties of hue, of tint, and of shade, their melodies, so to speak, and their harmonies, all combine to render them sources of delight.

What would the landscape be without colour? Were it composed of only lights and shades, it would lose far more than half its beauty. It would be like a print compared with the glowing tints of a Claude or a Turner. What would be the plumage of the peacock without its gorgeous colours—its brilliant lustre, its playful hues? Let that ghost-like variety which is colourless say. And the most lovely of God's creatures—without colour, how would she appear? Where were the rosy cheeks, tokens of health—the coral lips—the many-hued iris, that index of the soul, with its deep yet lustrous browns, its ethereal blues, its tender hazels, its sagacious greys, with its margin of lucid white, the peculiar adornment of the human eye? And the wavy tresses too, with their tints in such strange sympathy with those of the iris—either in pleasing harmony or not less pleasing contrast. A woman of living alabaster, however elegantly formed, would hardly send a thrill of warmth through the frame of admiring man.

While colours thus afford pleasure to the eyes of the multitude, they awaken in the mind of the philosopher, who contemplates them with intelligent scrutiny, a still more exquisite delight. For he perceives in them evidences of most marvellous wisdom and skill, united to overflowing goodness and benign sympathy. When he considers the simplicity of the means, and the wondrous beauty and variety of the effects, he becomes lost in amazement. He feels himself, as it were, in the presence of a mind transcendently powerful, wise, and benevolent, so that his soul becomes filled with reverential, yet loving awe. For all these phenomena, which produce in him the varied and pleasurable perceptions of colour, are in themselves nothing more than variations in the rate of infinitesimally minute tremors, regulated by determinate mathematical laws.

The nature and minuteness of these vibrations, and some

of their regulating laws, have been indicated in a previous essay, entitled "Molecules, Ultimates, Atoms, and Waves," which appeared in the "Quarterly Journal of Science," vol. i. N.S., p. 170, in April, 1871, and the two following numbers. In that essay reasons were adduced for concluding that the bright coloured lines observed in the spectra of glowing gases are due not to the vibrations of the ultimates of the gases themselves, but to those of more minute atoms constituting those ultimates. More especially in the case of hydrogen, it was shown to be probable that the ultimate of that gas consists of four species of extremely minute atoms, whose separate vibrations produce the four bright lines which characterise the spectrum of that gas when made to glow by passing through it an electrical discharge.

These views, respecting the constitution of hydrogen and the other chemical elements, have received a remarkable confirmation in certain phenomena observed by the spectroscope in the solar chromosphere. When viewed with that instrument, the chromosphere usually presents the four lines characteristic of hydrogen, and two other lines—one in the yellow, not coincident with the sodium lines, nor with any other produced by any known terrestrial substance, and denoted as D_3—the other in the red, a little less refrangible than C, and in like manner not referable to any known substance. Now, in two observations—one by Mr. Lockyer, the other by Professor Young (the latter made on 19th April, 1870), the line F, supposed to be due to hydrogen, was agitated in a remarkable manner, indicating that the substance in which this line has its origin was in a state of violent commotion; but on both occasions the red line C, also supposed to be due to hydrogen, remained totally unaffected.

Of this remarkable phenomenon the most simple explanation would be to suppose that, in the chromosphere, the four atoms constituting the ultimate of hydrogen exist disunited, forming four distinct gases more subtle than hydrogen; that of these gases, the one producing the line F was, during the observations in question, ascending in a gyratory column, while the one producing the line C was at rest.

A similar conclusion may be drawn from other spectroscopic observations of the solar limb, in which certain of the dark lines of the spectrum become converted into bright lines. It is remarkable that only a certain number of the lines due to particular metals have been thus affected—more especially three of the lines referred to magnesium, and only one or two of the numerous lines referred to iron. The lines

thus altered were the three magnesium lines b_1, b_2, b_4, the line b_3 referred to iron and nickel, and the line 1474 of Kirchhoff's map also due to iron. But the strong magnesium line 5527 of Angström's scale and the numerous other iron lines underwent no similar change. It might be hence fairly inferred that, in the photosphere, the atoms constituting the ultimates of the various chemical elements, whose characteristic lines have been detected in the solar spectrum, all exist in a disunited state, forming an intimate mixture of highly attenuated gases; but that some of those gases occasionally pass the limits of the photosphere, and are projected a short way into the chromosphere, where they glow under the influence of electrical currents. For the bright lines, thus forming reversals of some of the dark lines of the photosphere, are always much shorter than the other bright lines of the chromosphere—a fact indicating that the substances which produce them ascend only a short way beyond the usual limits of the photosphere, in which these same lines are dark.

Subtle as, according to this view of their constitution, these gases must be, they must be excelled in their tenuity by others, which, extending beyond the chromosphere, form the corona seen in total eclipses of the sun. It is remarkable that the spectrum of the corona, as observed by Professor Young during the total eclipse of 1869, consists of three bright lines, so nearly coincident with those observed by Professor Winlock in the spectrum of the aurora borealis as to leave little doubt of their identity—thus indicating that the gases constituting the solar corona exist also in the region at the outskirts of the earth's atmosphere, where the auroral flashes play. These spectral lines of the solar corona and the aurora borealis have not yet been identified with any known spectra produced by artificial means. But the extreme lightness of the gases producing them renders it probable that, like the gases of the chromosphere, they consist of separate atoms not united into the ultimate of any chemical element. Should the individual lines be hereafter identified with any of those embraced in the spectrum of any known element or elements, this view of their constitution would be confirmed. (See Schellen's "Spectrum Analysis," pp. 361, 399, 404, 414).

The remarkable circumstance that, in the spectra of several of the nebulæ, there is seen only one of the bright lines of hydrogen—that, namely, corresponding to the line F—might in like manner be explained by supposing that, in these nebulæ, the atoms composing the ultimate of hydrogen

are separate from each other, and that only those atoms yielding this particular bright line are in sufficient quantity to originate a light of such intensity as to penetrate through so great a distance as that at which these nebulæ are placed.

This particular phenomenon it has been sought to explain by the fact, observed by Messrs. Frankland and Lockyer, that when attenuated hydrogen is illuminated by the electrical discharge, and the spectrum produced is viewed at a considerable distance, all the bright lines disappear, save that corresponding to F. But this explanation is inconsistent with the fact that all the four hydrogen lines are distinguishable in the spectrum of the solar chromosphere. Moreover, the disappearance of all the lines but F from the spectrum of attenuated hydrogen, when viewed at a distance, is a fact which itself requires explanation; and the simplest is afforded by the supposition that, in each ultimate of hydrogen, the atoms which vibrate in unison with the line F considerably exceed in number those which give rise to the other three bright lines. It appears very unlikely that, were all the ultimates of hydrogen themselves individual atoms of the same bulk and weight, a greater number of them should elect to vibrate in unison with the F line than with the three other bright lines; while we should be left without any assignable reason why such atoms, if all exactly alike, should not, every one of them, vibrate in exactly the same time, and so give rise to only one bright line.

It has been recently pointed out by Mr. G. Johnston Stoney that the wave-lengths of the 1st, 2nd, and 4th hydrogen lines stand to each other approximately in the following relation—$20H_1 = 27H_2 = 32H_4$; whence he infers that these three may be harmonics derived from one and the same fundamental vibration (Phil. Mag., Aug., 1868). To this conclusion, however, is opposed the fact of the preponderance of H_2 or F over all the others; and still more the phenomenon already noted that, in two distinct observations on the solar prominences, the line H_2 or F was violently agitated, while H_1 or C remained unaffected. These two facts it appears impossible to explain, except on the supposition that C and F have their origin in two distinct sets of atoms, capable of existing either separately, constituting different gases, or united into one ultimate—that of hydrogen gas. In the sequel it will be shown on other grounds to be extremely improbable that these two have their wave-lengths in the exact ratio of $20C = 27F$. There remains, moreover, the fact that the vibrations corresponding to the line H_3 have no such numerical relations to the other three

as these last have approximately among themselves, and exhibit no indication of their being derived from one and the same fundamental vibration with them. It appears, therefore, safest to conclude that the approximate numerical relation $20H_1 = 27H_2 = 32H_4$ is simply an indication that the inertiæ of these three sets of atoms stand to each other nearly in this relation.

It seemed advisable to make these explanations in reference to the former essay on "Molecules, Ultimates, Atoms, and Waves" with a view further to illustrate the subject of which it treats. It is now proposed to consider more at large the phenomena of colour simply as they present themselves to the eye, with their various relations.

Colours may be divided into two great classes—intrinsic and adventitious. Intrinsic colours depend on the arrangement of the molecules, ultimates, or atoms constituting the coloured substance; while adventitious colours depend on the disposition of aggregations of these into grains, fibres, layers, prisms; or they are due to the interference of wave with wave, the superposition of wave upon wave, the separation of wave from wave of the luminiferous ether; also in some cases to an alteration in the rate of vibration of the ethereal waves.

Intrinsic colours first demand attention, the phenomena which they present being comparatively few and simple. In the case of an elementary substance, which, while in the state of gas or vapour, exhibits colour, such as chlorine gas and the vapours of iodine and bromine, the colour most probably depends on the arrangement of the atoms constituting the ultimates of those elements. And this phenomenon furnishes a strong argument in favour of the view, that the chemical elements are really compounded of still more simple atoms. Did the colours of those elemental vapours depend on vibrations performed simply by their ultimates, seeing the vibrations would, in that case, be all of one rate, the tint produced would be one or other of the pure unmixed colours of the spectrum. But this they are not, consequently the vibrations causing them must be of various rates; nor does it appear possible to find any other cause of such a variation of rate than that of their being due to the compound nature of the ultimates—their consisting of atoms which, when set in motion by the ethereal waves, vibrate at different rates, producing a compound tint.

When the chemical elements are not in the gaseous or vaporous condition, their colour probably depends on the arrangement of the ultimates and their rates of vibration,

rather than on those of the atoms constituting the ultimates. How much depends on the arrangement of the ultimates and their state of aggregation has been rendered evident by Faraday's experiments on gold leaf. This metal, when in very thin layers, is transparent, and the light passing through it is green; but by heating such films, and so altering the state of aggregation of the ultimates, the colour of the transmitted light becomes ruby-red. It can, however, be restored to green by simply compressing the layer. The light from the surface of the film in both cases retains its rich yellow hue and beautiful metallic lustre. In toning photographs with gold, however, the film, when extremely thin, is black, and not till the thickness of the deposit is augmented to an appreciable extent do the yellow tint and the metallic lustre return—the lustre preceding the tint in its reappearance, so that, at a certain stage, the surface presents a certain amount of metallic lustre while it is still black. Other metals besides gold exhibit variations of tint depending on the state of aggregation of their ultimates.

These phenomena bring us face to face with the question relative to the nature of intrinsic colours—the manner in which they are produced by the action of the ethereal vibrations. At one time it was generally supposed that the light falling on any coloured surface becomes separated into two portions, of which one is regularly reflected without change, the other scattered in all directions by the reflective action of the molecules or ultimates of the coloured surface, but deprived of some of its waves by absorption. Another opinion, however, has begun to prevail over this first notion. When it is remembered that what arrives at the coloured surface is simply motive energy, wafted onwards through the ether in waves of definite length, embracing vibrations of various rates, it will be perceived that if any of the motive energy of the ether disappear or become absorbed, it must be imparted to the molecules or ultimates of the surface on which the waves alight. These, again, cannot take up the energy without being themselves set a vibrating at the peculiar rates which they tend to assume. Moreover, the molecules or ultimates, on beginning thus to vibrate, must excite in the ether, in immediate association with them, fresh vibrations synchronous with those peculiar rates, and these will be propagated by undulations in all directions. It is this secondary set of ethereal vibrations which, according to the second view, produce in us those perceptions which we call the intrinsic colours of bodies. It is not a part of the incident light deprived of certain of its component waves,

and scattered by reflection in all directions; but it is an entirely new set of waves owing their origin to the vibrations of the molecules or ultimates established by the motive energy of the incident light—these vibrations being of the same rate as those producing certain colours when they subsist in the ether.

Among other phenomena which favour this latter view is that presented by the scarlet geranium. It has long been observed that the colour of that flower continues to glow with apparently deeper intensity in the twilight. Now were the colour produced by the scattering in every direction of a portion of the incident light deprived of all its constituent waves, save those which combine to produce scarlet, the colour ought to become sensibly weaker as the incident light diminishes. But its appearing more intense, after the incident light has been greatly weakened, tends to prove that the scarlet colour is really produced by the vibrations of the colouring-matter of the petal—these vibrations subsisting for a considerable time after the stimulus of the incident light is lessened, and generating by their reaction vibrations in the ether synchronous with themselves. The apparent increase of intensity in the twilight is due to the circumstance that the scarlet colour is then less diluted with that portion of the incident light which is actually scattered in all directions from the surface of the petal during sunshine.

In the majority of cases the nature of the action is masked by the circumstance that the molecules or ultimates cease to vibrate almost immediately after the stimulus of the incident light ceases, though some wall-papers show their colours for a few seconds after the extinction of a candle, which has been placed near them. Nor is this owing to the mere persistence of the image on the retina; for it continues after the brighter image of the candle itself has disappeared. The same view is also strengthened by the phenomenon of lustre. For lustre is simply a portion of the incident light scattered from the coloured surface in every direction; but it is quite distinguishable from the coloured light of the surface itself, generated by the vibrations of the particles of which the coloured body is composed.

In all inorganic bodies the intrinsic colour is for the most part equably distributed over the surface or throughout the mass. In some chemical compounds the different ultimates constituting the compound molecule vibrate at different rates; but these become so blended as to produce compound tints; and they cannot be separated by submitting the substance to microscopical examination.

With organic bodies it is otherwise. Among these, the cellular structure more or less modified is so prevalent, that it is not surprising to find that their colouring-matter tends to accumulate in cells, which are easily distinguishable under the microscope. These are termed pigment cells. Even in cases where to the naked eye the tint appears uninterruptedly continuous and uniform, the microscope shows this apparent uniformity to be due almost entirely to the minuteness of the pigment cells and their close aggregation. Nothing can appear to the naked eye more uniform than the beautiful crimson tint of certain portions of the petal of the pelargonium, yet under the microscope the colour is seen to be accumulated in curiously-formed pigment cells. So, also, the skin of the negro, which to the naked eye appears of a uniform very dark brown, is seen when examined by the microscope to have its brown pigment accumulated in cells —some large and of a crescent shape, others much smaller and round. Another beautiful example is furnished by the minute sea-weed *Polysiphonia vestigiata*, which appears of a uniform red tint. Under the microscope the red pigment is seen to be accumulated in cells of an elongated form arranged in successive stages, a peculiarity from which the plant derives its name.

It has been mentioned that in the case of gold-leaf, the light transmitted through the film has a different colour from that which comes from its surface. This phenomenon, termed "dichroism," is exhibited by several other substances—silver-leaf, for instance, transmitting a blue light, while that proceeding from its surface is nearly white. The mineral termed dichroite or iolite, a prismatic quartz, is another example, its colour being deep blue when viewed in the direction of the axis of the crystal, and yellowish grey in the transverse direction. Crystals of augite, again, are blood-red in one direction and bright green in another. The alcoholic solution of chlorophyl, or leaf green, tinges the light passing through it of a deep red, while the superficial colour is green. In tincture of litmus the transmitted colour is also red, but the superficial is blue. The change from green to red in the instance of gold-leaf shows that, in some cases, these transmitted tints depend simply on the state of aggregation of the constituent ultimates. But in the tinctures of chlorophyl and litmus, the transmitted red is due to one of the constituents of those chemical compounds.

An interesting case is presented by the tincture of the bark of horse-chesnut, for it is one of transition. While dichroism may be regarded as intermediate between ordinary

intrinsic colour and fluorescence, this tincture exhibits the transition between fluorescence and dichroism, another example of that tendency to gradation so conspicuous in many natural phenomena. In some varieties of fluor-spar there is dichroism combined with fluorescence, the green and blue fluor imparting to the light transmitted through it a green colour, while the superficial tint is deep blue. In the solution of the disulphide of quinine, again, there is fluorescence without dichroism, the transmitted light being colourless, and only the superficial light exhibiting the blue tint due to its fluorescent property. In uranium glass we have again dichroism combined with fluorescence, the transmitted light being yellow, the superficial fluorescent tint blue. But in the tincture of the bark of the horse-chesnut, when dropped in small quantity into water, there is a curious combination and succession of effects. The transmitted light is at first colourless, as in the case of the quinine solution, while the superficial tint is blue, but deeper than that proceeding from quinine. In a short time, however, the transmitted light in the case of the horse-chesnut bark acquires a straw-colour, which gradually deepens, the blue fluorescence still continuing without much diminution, so that we have again dichroism and fluorescence combined. Ultimately, however, the solution, though exceedingly weak, acquires the tint of brown sherry as respects both the transmitted and the superficial light, the fluorescent blue having gradually died away.

Fluorescence itself forms the transition between intrinsic and adventitious colour. There can be no doubt that this phenomenon is caused by the vibration of the molecules of the fluorescent body. The peculiarity is that this motion may be established by ethereal waves lying beyond the limits of the visible spectrum. The most remarkable case is that presented by the extremely minute ethereal waves proceeding from aluminium electrodes, which, as has been shown in a previous essay already referred to, are very far removed from the visible spectrum beyond its violet extremity—beyond even the limits of actinic action. Yet these minute waves can excite in the phosphate of uranium vibrations which, in their turn, originate fresh ethereal vibrations lying within the limits of visibility. It is thus rendered evident that the vibrations excited in the uranium salt are very much slower in their rate than are the ethereal vibrations by which they are established, and that these uranium vibrations in their turn give rise to fresh ethereal vibrations synchronous with their own slower rate,

and capable of exciting the optic nerve. The case resembles that of a bass string set a vibrating by the vibrations of a treble string several octaves higher in the scale.

This phenomenon affords evidence that the molecules of bodies are actually made to vibrate by the ethereal waves, and do in their turn propagate a secondary set of ethereal vibrations—so far favouring the second view of the nature of intrinsic colours. Indeed, according to this view, the only difference between fluorescent colours and ordinary intrinsic colours consists in this circumstance, that, whereas the latter are due to vibrations established in the molecules by ethereal waves lying within the limits of the visible spectrum, the vibrations causing fluorescence are established by ethereal vibrations more rapid—sometimes greatly more rapid than themselves. If the incident light be winnowed from all waves of shorter period than the green, there is no fluorescence; and in this sense the fluorescent tint may be regarded as adventitious; because it depends for its exhibition on the character of the incident light. But it is in another aspect intrinsic; because it depends on the molecular vibrations of the fluorescent body.

The flame of a spirit-lamp, though deficient in light capable of stimulating bodies to exhibit their intrinsic colours, abounds in that sort of light which stimulates fluorescent bodies. If in a dark room a spirit-lamp be lighted and placed behind the observer, and if he put on a smooth black surface a drop of water, and alongside of it a drop of a weak solution of the disulphide of quinine, or of the bark of the horse-chesnut, and examine these by placing them near the level of the eye, while the drop of water will be hardly visible, that of the fluorescent liquid will appear quite solid and of the colour of a turquois.

Another phenomenon, illustrating the great influence of the molecular condition of bodies upon the light falling on them, is that of temporary colours. The most familiar example of these is furnished by the sympathetic inks formed by the chlorides of cobalt and nickel. Very dilute solutions of those salts are so nearly colourless that when laid on paper they are invisible. But when subjected to heat the former becomes blue, the latter yellow, while by combining the two a green is obtained. These colours gradually disappear when the paper stained with them is exposed to the air; but they may be restored again and again by mere warmth. The explanation is that the heat drives off all moisture from the salts, and their molecules when dry tend to vibrate—the one in unison with the blue,

the other with the yellow ray, when exposed to light. On the withdrawal of the heat the salts again imbibe moisture from the air, and their molecular vibrations, under the stimulus of the incident light, have no longer these definite rates.

The temporary effects of heat on nitrous acid gas may be classed under this same head. This gas, even at ordinary temperatures, exerts on the incident light a strong absorptive action, in virtue of which numerous dark lines are developed in the spectrum; but raising the temperature of the gas so increases this absorptive power as ultimately to convert the whole of the incident light into dark radiant heat—the gas becoming quite opaque. A fall of temperature allows it to resume its transparency. In this case, the heat tends to cause the molecules of the gas to take up the vibratory energy of the incident light, and in virtue of this energy, united to that of the applied heat, to perform vibrations of so great an amplitude and so slow a rate that they do not in their turn communicate to the ether back-waves of a rapidity sufficient to affect the optic nerve. These back-waves accordingly assume the form of dark radiant heat. When the temperature is lowered again, the molecules perform vibrations of smaller amplitude and greater rapidity, which in their turn propagate through the ether back-waves of such rates as to develop colours belonging to the red end of the spectrum.

PART II.

Intrinsic colours having been considered in the previous part, the present shall be devoted to those called adventitious. Of such, the most simple sort are those produced by dispersion, or the separation of wave from wave of the incident light. In this case, the medium by which the separation is effected may itself be destitute of colour. All that is requisite is that it should be shaped into the form of a wedge or prism, so that the incident light shall pass through varying thicknesses. The diverse waves, of which the incident light consists, are thus subjected to the retarding action of the medium for different periods of time; and they are accordingly turned aside out of their direct course, or refracted in unequal degrees. Those most easily retarded become thus separated from those least easily retarded, and the waves of different lengths reach the eye in this separate condition, producing each its distinct impression of colour. All refracted spectra are of this character. The colours do not belong intrinsically to any

substance or object. The eye which is usually impressed simultaneously by luminous waves of every degree of length, causing the perception of mere brightness, is in this case separately impressed by waves of different definite lengths, the vibrations of which are so adjusted to those of which the optic nerve is capable, as to excite in us the perception of definite colour.

The laws which regulate the dispersion of light in passing through diverse media are exceedingly curious; and some of the more important of them have been noticed in a previous essay on the spectroscope in the "Quarterly Journal of Science" for January, 1872.

A familiar example of the production of adventitious colours by the separation of wave from wave of the ether, where the object which affects the separation is itself colourless, is exhibited in the rainbow. This phenomenon is produced by the action of falling rain-drops, or of the spray from waterfalls on the sunbeams. The ethereal waves, on entering a rain-drop, become separated one from another, owing to their unequal refrangibility, and their passing through different thicknesses of the watery medium. Being reflected from the posterior surface of the drop in this separate condition, they undergo further separation in passing a second time through the water; so that, on emergence, the differently coloured waves reach the eye separately. The mode of formation of the primary and secondary bows will be found explained on mathematical principles in the "Edinburgh Encyclopædia," vol. xv., p. 616.

Haloes round the sun and moon are also examples of the same sort of adventitious colours, and their explanation depends on similar principles; only the objects by which the separation of the ethereal waves is effected are thought to be not rain drops, but minute frozen particles of water. It is supposed that a stream of air, charged with moisture at a low temperature, comes into contact with a denser, drier, and colder stratum, by which the particles of moisture become suddenly frozen into very minute crystals, which are sustained floating in the atmosphere at a considerable height, in a thin semi-transparent layer, forming a sort of veil between the observer and the sun or moon. The explanation of these phenomena on mathematical principles will be found in the "Edinburgh Encyclopædia," vol. x., pp. 616, 617.

To similar causes are to be attributed the rarer and more striking phenomena of the parhelion or mock-sun, and the paraselene or mock-moon. The author, many years ago, once enjoyed an opportunity of seeing a parhelion of great

beauty in the north of Scotland, and he still retains a lively recollection of its aspect. The phenomenon varies considerably, but in general it may be said to consist in the formation of several large luminous rings or arches, sometimes coloured, sometimes only bright, at some distance from the sun or moon, and intersecting each other at two or more points—the points of intersection being usually occupied by the mock-sun or mock-moon. Sometimes there are only two of these spectral images of the luminary—one on either side of the true disc, and at a considerable distance from it. In other instances there are three or four—more rarely six such spectral images. To this latter category belonged the remarkable parhelion seen by Scheiner in 1630, of which a particular description was handed down by Gassendi, the astronomer. See the "Edinburgh Encyclopædia," vol. x., p. 613, where several other forms of the phenomenon are described. In that of 1630 there was one complete luminous ring around the sun, another much larger passing through the disc of the luminary, a third of nearly the same size surrounding the sun, but of which the lower third was invisible; while there was a portion of a fourth touching the upper limit of the third, and stretching thence upwards a short way towards the zenith. Of the spectral images of the sun, four were situated in the large ring passing through his true disc. They were formed at the points where this ring was intersected by the other two, which had the true sun for their centre. The fifth image was situated right over the true sun, on the margin of the innermost of those two surrounding rings; while the sixth was situated also right above the true sun at double the distance from his disc, on the margin of the second surrounding ring, at the point where it was cut by the fragmentary ring at its summit. The spectral images seen by Scheiner continued visible for upwards of four hours.

The second case of adventitious colour is that due to the interference of one luminous wave with another—the two proceeding from very closely approximated surfaces. The system of rings, named after their discoverer, Sir Isaac Newton, presents this phenomenon in its simplest form. To obtain these in perfection, it is necessary to place a long focused convex lens against a little longer focused concave lens, and to exhaust the air from between them, so as to press them very closely and equably together by atmospheric pressure. The colours of the reflected and transmitted light are in every case complementary to each other, being such as would, by their union, produce white light. When

light of one pure colour is thrown on the lenses, the rings are all of that one colour, and merely light and dark—the waves alternately doubling and extinguishing the effects of each other. Their breadth is greatest with red and least with violet light; while it is by the overlapping of these rings and the consequent intermingling of their tints that the succession of colours is produced when white light is employed.

Another method of exhibiting these beautiful rings is by blowing soap bubbles of a large size. This may be done by using a mixture of soap and glycerine, and the bubbles thus obtained may be preserved for several hours intact under a bell-glass. The colours are here produced by the interference of the light coming from the inner surface of the film with that coming from its outer surface. The two surfaces are most nearly approximated at the summit of the bubble, and they gradually separate thence downwards, so that the same conditions are present as in the case of the two lenses. Another simple way of producing this system of rings is by spreading a thin film of soap over a glass plate, and breathing on it through a finely pointed metal tube. In this case the effect is due to the condensation of the breath into minute hollow vesicles, which increase in size from the centre outwards. They are, in fact, diminutive soap bubbles.

This class of colours goes under the general denomination of the colours of thin plates, and the colours of many natural objects fall under this category. Among the most beautiful of these, and the most nearly allied to Newton's rings, are the colours exhibited by the discoid frustules of certain of the Diatomaceæ. These consist of very thin superimposed plates of pure silica, ornamented with various patterns, produced by extremely minute papillary projections.

To this same class belong the colours seen in the scum floating on the surface of some liquids, especially of solutions containing salts of iron; also the colours of fibres and of feathers very generally. The colour of some feathers, however, are intrinsic, consisting of colouring-matter lodged in pigment cells, whence it can be removed and separately examined. The most interesting case of the kind is that of the red feathers in the wings of the plaintain-eater (*Musophaga violacea*) and the turacu (*Turacus albocristatus*), which owe their red colour to a pigment that has been named turacine. This pigment possesses dichroism, being of a deep violet purple by reflected light and crimson by transmitted light. It presents the great peculiarity of containing nearly 6 per cent of metallic copper, which must have entered with the

food or drink of the bird, have passed through its circulation, and found its ultimate lodgment in those wing-feathers. It is most abundant at the pairing season (see an interesting paper on the subject in "The Student," vol. i., p. 161, where will be found a chromolithograph of the two birds above named).

All iridescent colours in fibres or spines are adventitious, and belong to the class of colours of thin plates; as, for example, the beautiful iridescent spines of the sea-mouse (*Aphrodite aculeata*), and the iridescent branchiæ of the *Eolis*, which serve the double purpose of a breathing apparatus and a bank of oars. The colours of the wings of insects and the elytra of beetles, &c., all fall under the same extensive category.

The iridescence of mother-of-pearl and the fire of the opal, again, though also phenomena of interference, may perhaps be regarded as rather transitional in their character, approaching towards the colours developed by systems of fine lines, producing the phenomena of diffraction. The simplest case of diffraction is that of the external and internal fringes, developed when a single thin obstacle, such as a fine wire or a very thin opaque plate placed edgewise, is set in the path of a divergent beam of sunlight. In this instance the internal fringes are produced by the overlapping of the waves bent inwards from the opposite sides of the obstacle; while the outer fringes are due to secondary waves propagated from the outer edges of the obstacle, which interfere with the direct waves coming from the luminous source. It is by a system of extremely fine and very closely approximated equidistant lines that the diffracted spectrum—the purest of all spectra—is produced.

By far the most beautiful exhibition of adventitious colours is that to be obtained by means of polarised light, or light consisting of waves, the vibrations of which are all performed in one plane. To produce the phenomena of colour in this manner, it is needful to have the means of polarising the light in two opposite planes—the plane in which the vibrations are performed in the one set of waves being perpendicular to that in which they are performed in the other set. The light may be thus polarised either by reflection from a smooth surface at a certain angle, or by means of crystals of Iceland spar, cut so as to form what are called, from their inventor, "Nicol's prisms," or else by means of a plate of tourmaline or of iodide of quinine. Of these appliances one is used for polarising the light, the other as an eye-piece for analysing it, that is to say, for showing

that it is polarised, and for indicating the character of its polarisation. The colours are developed when certain crystals and also certain organic substances are interposed between the polariser and the analyser. In passing through these interposed media, the light is more or less depolarised, while the depolarising energy acts unequally on the different waves, and is manifested unequally in different parts and directions when the interposed medium is a crystal. The result is the greater or less separation of the differently coloured waves one from another, and that in such a manner as, in many instances, to display the intimate internal structure of the crystal, or other depolarising substance.

The combined action of different colours when they fall simultaneously on the retina is curious. An interesting series of experiments, with a view to illustrate this action, has been made by Prof. J. Clerk Maxwell, who has communicated the results to the Royal Society in a paper published in the "Philosophical Transactions" for 1860. By an ingenious apparatus he contrived to bring three diverse pure colours of the spectrum to bear on one point of the retina. He ascertained that there is in the spectrum a central point, which he describes as being about a fourth from E towards F. This would make its wave-length on Angström's scale 5156·72. As Prof. Maxwell determined this point by means of two flint glass prisms, allowance must be made for their irrationality; so that in all probability the exact position of the central point is, in the normal spectrum, the mean green ray, of which, as will be afterwards shown, the wave-length is 5124·086 (reciprocal 1951·568). Prof. Maxwell has detected a curious peculiarity of the rays at and near this point, namely, that at the *punctum cœcum*, or yellow spot in the retina, there is a greater insensibility to these rays than to any others of the spectrum.

By causing the rays from this central green point to fall on the retina in conjunction with the rays from some point in the red, Prof. Maxwell found that colours undistinguishable from the intermediate pure orange and yellow of the spectrum could be produced, the only difference being that these compound tints are resolvable by the prism into their constituent elements, while the pure tints of the spectrum are not. In like manner it is always possible to select two colours from somewhat distant points of the spectrum, which will, when combined in certain proportions, produce intermediate tints undistinguishable from one or other of the remaining pure tints of the spectrum.

The most remarkable effects, however, are those produced

by causing the rays from three points in the spectrum to fall on one point of the retina,—the result being the impression of pure white, undistinguishable from the white resulting from the combination of all the spectral colours. The proportions required to constitute this white vary with the points on either side of the central green from which the rays are taken. It is impossible by mixing pigments to produce a similar result. If carmine, chrome-yellow, and indigo be mixed in certain proportions, the resulting impression on the eye is that of blackness, not of whiteness. It is possible, indeed, by whirling a disc, painted with different proportions of red, green, and blue, to produce a greyish-white, but not that pure white which may be obtained by combining the rays of the spectrum.

Prof. Maxwell extended his observations to the case of colour-blind persons. The eyes of those whom he examined were dichromic,—that is, sensible of only two impressions of colour. The central green of the spectrum appeared to them white, as did a considerable extent on either side of it. Beyond that, on the less refrangible side, all appeared of one colour, which they termed yellow of different degrees of intensity, shading off into darkness towards the red extremity; while on the more refrangible side all appeared likewise of one colour, which they called blue of different degrees of intensity, shading off into darkness at the violet end. The space from the fixed line A to E appears yellow, reaching its maximum between D and E, while the blue reaches its maximum at about two-thirds from F towards G. The mean green ray produces a fainter impression on the *punctum cæcum* in such eyes than in those of more perfect visual power. In the dichromic eye, rays taken from different points of the regions on opposite sides of the central green, when combined in certain proportions, produce the impression of whiteness without the aid of a third ray. But no admixture of blue and yellow will to such eyes appear green. Any combination of these two will appear white,—either a yellowish-white if the yellow be in excess, or a bluish-white if the blue predominate.

These experiments throw great light on the nature of complementary colours. They show that to perfect eyes, when two colours are complementary, one or both of them must be compound colours, and that only in dichromic eyes can two pure colours be regarded as complementary. To such eyes, yellow and blue being the only colours distinguishable, are always complementary to each other. When a perfect eye, however, after dwelling for a long time on a

pure red, is turned on a pure white, the complementary green which it sees is not pure, but is that mixture of blue and green which is needful to complement the pure red, in order to constitute a perfect white. So, when the eye first dwells on a pure green, the red which it subsequently sees is not pure, but that mixture of red with blue which, in order to constitute a perfect white, is needful to complement the pure green. From these observations it follows, that in Newton's rings, the reflected and refracted tints, being complementary to each other, cannot be pure colours, such as are those of the diffracted spectrum; but there must be at least three pure colours in every opposing pair of the Newtonian rings.

From the foregoing sketch it will be perceived what an additional charm has been thrown around the subject of colour by the discoveries of Natural Philosophy. By the appliances of which that science avails itself we are, as it were, furnished with additional organs of vision, and enabled to contemplate natural beauties, of which the human mind had, before those discoveries, hardly formed a conception. And then there returns upon us the startling fact, that all these wonderful and beautiful phenomena are nothing more than mere variations in the rates of certain minute vibrations,—just as are the notes of various musical instruments in the case of sound, whose melodies and harmonies have thus, to a certain extent, their analogies in those of colour. The nature and scope of these analogies will be considered in the remaining part of this paper.

Part III.

The analogy between colours and musical tones has presented itself to many minds, and there has been among scientific men much discussion as to its nature and extent. The grounds on which those who have contended for a perfect correspondence between the colours of the spectrum and the notes of the musical scale have based their argument, were at one time supposed to be stronger than they actually are.

The case is greatly complicated by the uncertainty which prevails in regard to what really constitutes the true musical scale. The mathematical idea of a perfectly musical scale is one that should divide the octave into twelve equivalent semitones, forming a regular geometrical progression. For the purpose of comparison with the actual musical scales, this ideal scale is here given, with the relative number of

vibrations referred to those of *do* as unity, and with the logarithms of these numbers, the common ratio of the progression being the twelfth root of 2.

Ideal Scale.

Names of Notes.	Ratios of Vibrations.	Logarithms.	Com. Dif.
Do	1	0·0000000	0·0250858
Do♯ re♭ .	1·059463	0·0250858	
Re	1·122462	0·0501716	
Re♯ mi♭ .	1·189207	0·0752574	
Mi	1·259921	0·1003432	
Fa	1·334839	0·1254290	
Fa♯ sol♭ .	1·414213	0·1505148	
Sol . . .	1·498306	0·1756006	
Sol♯ la♭ .	1·587400	0·2006864	
La	1·681792	0·2257722	
La♯ si♭ .	1·781796	0·2508580	
Si	1·887747	0·2759438	
Do_2	2	0·3010300	

Thus constituting a regular geometrical progression.

Had the earliest musicians been also mathematicians it is not improbable that this is the scale they would have adopted; while so great are the powers of habit and inheritance on man's mind and organisation that it would, in the course of time, have come to be regarded as the true scale, the succession of its notes as perfect melody, their combinations as perfect harmony. The state of the fact, however, is quite otherwise. Melody and harmony have become, to a certain extent, dissociated, and the scale which is regarded as yielding the most perfect melody differs from that which is regarded as yielding the most perfect harmony,—neither of them, however, forming regular geometrical progressions, consequently both differing considerably from the ideal scale.

In the Pythagorean scale, which yields the most perfect melody, the *sol* is regarded as occupying the exact middle point between *do* and its octave do_2; consequently the ratio of its vibrations referred to *do* as unity is 1·5. From these three, *do, sol*, do_2, all the other members of the scale are derived by multiplication or division. The principal notes are found thus:—$Sol^2 \div do_2 = re$, $re^2 = mi$, $sol \div re = fa$, $mi \times fa = la$, $mi \times sol = si$. The sharps thus:—$Fa \div mi = do\sharp$, $sol \div mi = re\sharp$, $re\sharp^2 = fa\sharp$, $re\sharp \times fa = sol\sharp$, $fa^2 = la\sharp$. The flats

thus :—*Mi* ÷ *re*♯ = *re*♭, *la* ÷ *fa*♯ = *mi*♭, *re*^{3} = *sol*♭, *re*^{4} = *la*♭, *re*^{5} = *si*♭.

This scale, with its logarithms and their differences, stands as follows :—

Pythagorean Scale.

Names of Notes.	Ratios of Vibrations.	Logarithms.	Differences.	Differences.
Do . .	1	0	0·0226335	
Do♯ . .	1·0535	0·0226335		0·0058856
Re♭ . .	1·067872	0·0285191	0·0226335	
Re . .	1·125	0·0511526	0·0226335	
Re♯ . .	1·185185	0·0737861		0·0058856
Mi♭ . .	1·201356	0·0796717	0·0226335	
Mi . .	1·265635	0·1023052	0·0226335	
Fa . .	1·33′	0·1249387	0·0226335	
Fa♯ . .	1·404663	0·1475722		0·0058856
Sol♭ . .	1·423829	0·1534578	0·0226335	
Sol . .	1·5	0·1760913	0·0226335	
Sol♯ . .	1·580207	0·1987248		0·0058856
La♭ . .	1·601808	0·2046104	0·0226335	
La . .	1·6875	0·2272439	0·0226335	
La♯ . .	1·77′	0·2498774		0·0058856
Si♭ . .	1·802034	0·2557630	0·0226335	
Si . .	1·898438	0·2783965	0·0226335	
Do_2 . .	2	0·3010300		

This is the scale according to which the violin is tuned and played, except when it accompanies a keyed instrument. It will be observed that the sharps and flats are here separated, and this distinction is recognised by all good violinists. The geometrical progression in this scale is far from perfect, but the irregularities are recurrent and nearly symmetrical. It has been shown, moreover, by MM. Cornu and Mercadier, that this scale agrees very closely with observation, when a violin is made to register automatically the vibrations of its strings. (See "Nature," vol. iii., 75).

It is found however, in practice, that the *mi* of this scale, when struck along with the *do*, does not produce perfect harmony, and that, to obtain a harmony free from beats, the *mi* must be lowered by a small interval called a comma, its value being 81÷80. This alteration in the value of *mi* involves an alteration in several other notes of the scale, in order to obtain good harmony; while it is also found most convenient to throw the adjacent sharps and flats together

into one note, as they exist in the ideal scale, for better adaptation to keyed instruments.

In the construction of this harmonic scale the same three notes, *do, sol,* do_2, are, as in the former case, assumed as a basis, *re* and *fa* being derived from them in the same manner as before. But the other notes are deduced from these on a different principle from that which is followed in the preceding case. There are formed three arithmetical progressions—*do mi sol, do fa la, re sol si*—the first having a common difference of 1-4th, the second of 1-3rd, and the third of 3-8ths, while from these *mi, la,* and *si* are respectively derived thus:—

$$Mi = do + \frac{sol - do}{2},\ la = do + \frac{fa}{2},\ si = 2\,sol - re.$$

The chromatic members of the scale are found thus:—*Fa* ÷ *mi* = *do*♯ or *re*♭, *sol* ÷ *mi* = *re*♯ or *mi*♭, *re* × *mi* = *fa*♯ or *sol*♭, *fa* × *mi*♭ = *sol*♯ or *la*♭, fa^2 = *la*♯ or *si*♭. The following is the scale thus constructed, with its logarithms and their differences:—

Harmonic Scale.

Names of Notes.	Ratios of Vibrations.	Logarithms.	Differences.	Differences.	Differences.
Do . .	1	0·	0·0280287		
Do♯ re♭	1·066′	0·0280287		0·0231238	
Re . .	1·125	0·0511525	0·0280287		
Re♯ mi♭	1·2	0·0791812			0·0177288
Mi . .	1·25	0·0969100	0·0280287		
Fa . .	1·33′	0·1249387		0·0231238	
Fa♯ sol♭	1·40625	0·1480626	0·0280287		
Sol . .	1·5	0·1760913	0·0280287		
Sol♯ la♭	1·6	0·2041200			0·0177287
La . .	1·66′	0·2218487	0·0280287		
La♯ si♭	1·77′	0·2498775		0·0231238	
Si . .	1·875	0·2730013	0·0280287		
Do_2 . .	2	0·3010300			

While the departure from a geometrical progression is in this case somewhat greater than in the Pythagorean scale, there is here more simplicity in the relation which the vibrations of each note bear to those of the tonic, whence probably its greater harmonic power. There is another result following from the departure from a regular geometrical progression, both in the case of the Pythagorean and the harmonic scale. According to the ideal scale, in which

the geometrical progression is regular, all major keys would have been exactly similar, and so would all minor keys. But the departures from regularity make every one major key to differ from every other major key, and so also with the minor, thus affording a much greater variety.

Now, as regards the correspondence of the scale of colour with one or other of the musical scales, it was at one time thought to be closer than it really is. For the rates of vibration corresponding to the junction of the colours were believed to constitute the following series,—1, 1·125, 1·2, 1·33′, 1·5, 1·66′, 1·77′, 2,—thus tallying with the harmonic scale in its minor mode. Prof. Listing, however, by a careful comparison of the most recent and accurate observations,—those made by Angström and others,—has determined, with a greater approximation to the truth, the wave-lengths corresponding to the borders of the several colours, and has shown that the reciprocals of those wave-lengths, which correspond to the ratios of the vibrations at those points, form a series approaching much more closely to an arithmetical than to a geometrical progression. (See Pog. An., vol. cxxxi., p. 564).

When the reciprocals of Prof. Listing's wave-lengths have their relations reduced to the simplest form, by making the smallest number = unity, they form the following series:—1, 1·117738, 1·235314, 1·352908, 1·470618, 1·588145, 1·705730, 1·823568.

There is here an evident approach to a common difference, of which the mean value is 0·117653.

This approach to an arithmetical progression becomes more apparent when the arithmetical means of the reciprocals of Listing's numbers are taken. These form the following series:—

Red.	Orange.	Yellow.	Green.	Blue.	Indigo.	Violet.
1,	1·111064,	1·222108,	1·333213,	1·444288,	1·555303,	1·666453.

It is evident what is the true law of this series, namely, that all of the above numbers should be perfect repeating decimals, having a common difference of 0·11′. This series, thus corrected, being assumed, it is easy to calculate backwards, so as to show the agreement of this assumption with observation. Taking the green as the central colour, by applying the above corrected series, we obtain from each of the other colours a value of the green; and the average of these six values will be found to differ by a mere trifle from the value deduced from the observations. From this corrected value of the green all the others may be found by

the above series, and the resulting values of the reciprocals of the wave-lengths for the mean colours will stand thus:—

Red.	Orange.	Yellow.	Green.	Blue.	Indigo.	Violet.
1463·676,	1626·307,	1788·937,	1951·568,	2114·198,	2276·829,	2439·460,

forming an arithmetical progression, of which the common difference is 162·631.

From the above, result the following numbers for the borders of the colours:—

1382·360, 1544·991, 1707·622, 1870·252, 2032·883, 2195·513, 2358·144, 2520·775,

—an arithmetical progression, of which the common difference is also 162·631.

From this last series we obtain the wave-lengths of the borders of the colours, in order to compare them with the wave-lengths given by Listing from observation. The following table gives the result:—

Observed.	Calculated.	Differences +	Differences −
7234	7234·09	0·09	
6472	6472·529	0·529	
5856	5856·1	0·1	
5347	5346·873		0·127
4919	4919·121	0·121	
4555	4554·744		0·256
4241	4240·624		0·376
3967	3967·034	0·034	
		0·874	0·759

These small differences are considerably within the limits of probable errors of observation, the more especially as Listing's numbers do not extend beyond four figures. It may accordingly be fairly concluded that, when reduced to their simplest form, by making the lowest number unity, the series will be for the borders of the colours—1, 1·117647, 1·235295, 1·352942, 1·470589, 1·588236, 1·705883, 1·832530; and for the mean rays of the colours—

Red.	Orange.	Yellow.	Green.	Blue.	Indigo.	Violet.
1,	1·11′,	1·22′,	1·33′,	1·44′,	1·55′,	1·66′

On comparing these two series with the three musical scales, it will be perceived that with these the first series has no points of correspondence whatever. Neither has the second series with the ideal musical scale. But in the two others the green corresponds exactly with the *fa*, while the violet tallies also with the *la* of the harmonic scale only. Moreover, if the violet be divided by the orange, the quotient,

1·5, will correspond to the *sol* of both scales. So, also, if the green be divided by the orange, the quotient 1·2, is equal to *mi*♭, or the minor third of the harmonic scale; while the violet divided by the green gives 1·25, corresponding to its major third.

Beyond these points of correspondence, in themselves not a little remarkable, there is no analogy between the scale of colour and the musical scales. The analogy is closest in the case of the harmonic scale; but there is this fundamental difference, that, whereas there are in that scale three interlaced arithmetical progressions, with diverse common differences, the colour scale consists of a single perfect arithmetical progression; so that, in their integrity, the two scales are irreconcilable. It is thus evident that the analogy between the two scales, so far from being perfect, consists only in this, that both are founded on a mathematical basis; but the colour scale forms a series much more simple and symmetrical than does either the Pythagorean or the harmonic musical scale.

These mathematical relations, subsisting among the mean rays of each pure colour of the spectrum, become all the more interesting when viewed in connection with those subsisting among the principal fixed lines of the normal spectrum, as respects their relative wave-lengths. For the purpose of a comparison of the one set of relations with the other, the latter may here be given as deduced from the very accurate observations of M. Angström. Assuming the more refrangible E as the centre of the system, and calling the value of its wave-length 10, the relative wave-lengths corresponding to the other fixed lines A, B, C, the less refrangible D, F, G, and the more refrangible H, may be found by the following formulæ:—

$$\begin{array}{lll}
(a) & 2A^2-6A \quad \ldots\ldots\ldots & = 3E^2+3E \\
(b) & (6A^2-A)-(6B^2+2B) \quad \ldots & = E^2+9E \\
(c) & (4B^2+2B)-(4C^2-2C) \quad \ldots & = E^2+E \\
(d) & (6C^2-C)-(6D^2+6D \quad \ldots & = E^2 \\
(f) & C^2+(2F^2-6F) \quad \ldots\ldots & = 2E^2+7E \\
(g) & (4G^2+4G)-(C^2+2C) \quad \ldots & = E^2+2E \\
(h) & (2F^2+6F)-(H^2+4H) \quad \ldots & = E^2+4E
\end{array}$$

The relative values of the eight wave-lengths, as given by observation, and as calculated from the foregoing equations, are as follows:—

	Observed.	Calculated.	Differences +.	Differences −.
A.	14·432490	14·432517	0·000027	
B.	13·033840	13·033839		0·000001
C.	12·454950	12·454930		0·000020
D.	11·189030	11·189003		0·000027
E.	10			
F.	9·225744	9·225760	0·000016	
G.	8·175214	8·175183		0·000031
H.	7·464880	7·464871		0·000009
			0·000043	0·000088

These trifling differences are much within the limits of probable errors of observation.

The foregoing seven equations give rise to the following more general one, embracing all the wave-lengths:—

$$a+b+c=d+f+g+h=6\mathrm{E}^2+3\mathrm{E}=630.$$

They also produce the following series:—

d	=	100	$c+f+g$	=	500
$a+h-f$	=	200	$a+f$	=	600
$b+c$	=	300	$a+c+g+h$	=	700
$a+b-g$	=	400	$2a+h$	=	800

These relations, taken in connection with the agreement between the wave-lengths calculated from the equations and those obtained from observation, render it in the highest degree probable that they have a true mathematical basis. They show that these wave-lengths are interdependent; so that no alteration can be made on any one of them without involving a corresponding change in all the rest.

Here the question arises—Do the intervals between any of those fixed lines among themselves, or between them and any other well known lines in the spectrum, correspond to any of the musical intervals, so as to render it probable that they are harmonically related? The first case that presents itself for consideration is that of hydrogen, in the spectrum of which occur two of the principal fixed lines, C and F, besides two other lines, designated as Hy_3 and Hy_4. As already mentioned, the wave-lengths of the three lines C, F, and Hy_4 stand to each other approximately in the ratio 20, 27, 32. Now as respects F and C, the foregoing formulæ may be applied to ascertain the accuracy of this relation between those two lines. Do they stand in the precise ratio 20 : 27 or 1 : 1·35? Taking the observed wave-lengths as given by Angström, the ratio is 1 : 1·350206. Taking the wave-lengths calculated from the formulæ, the ratio is

1 : 1·35017. But while in the first case the discrepancy might be attributed to errors of observation, it cannot be so attributed in the case of the calculated wave-lengths: for these could not be altered, even to so trifling an extent as to make the ratio exactly 1 : 1·35, without destroying the whole symmetry of the formulæ. It is accordingly far more probable that the ratio between F and C is as 1 : 1·35017 than exactly 1 : 1·35—a relation which would involve the conclusion that all the foregoing nicely balanced formulæ have no true mathematical basis, but arise out of mere arithmetical coincidences. Moreover, the ratio 1 : 1·35 does not correspond to that of any musical interval. The case is different with the relation between F and Hy_4, which is so nearly that of a Pythagorean minor third that the difference might be ascribed to errors of observation. If the calculated value of F be divided by the ratio of this minor third 32÷27, it will give for the wave-length of Hy_4 4101·258; whereas Angström makes the observed value 4101·2—the difference 0·058 lying much within the limits of probable error, so that the true relation between F and Hy_4 may very probably be that of this minor third. But it is the minor third proper to melody—not that proper to harmony, the ratio of which is 6÷5.

With respect to the other principal fixed lines, generally,—those namely embraced in the foregoing formulæ,—it may be affirmed that none of them stand to each other in a ratio corresponding to any of those found in the three several musical scales. Nevertheless, each of the lines stands in a relation of that kind to one or more other lines of the spectrum, within the probable limits of error of observation. These relations are shown in the annexed table, of which the first column contains the letter designating the fixed line; the second, the sign of multiplication or division. The next three columns the name of the musical interval in one or other of the three scales—the Ideal, the Pythagorean, the Harmonic, by which the wave-length of the fixed line is multiplied or divided. The sixth column contains the wave-length resulting from this multiplication or division. The seventh contains the corresponding wave-length in Angström's scale, to which it is nearest. The eighth shows the differences, plus or minus, between the two—these all lying within the limits of probable errors of observation. The ninth contains the names of the elements to which the wave-lengths are respectively due, and the tenth the colour of the region of the spectrum in which each wave-length occurs.

Looking to the general character of the relations exhibited in the table, they do not appear to encourage the supposition of their indicating that the lines thus connected correspond to rates of vibration, having their origin harmonically in one common vibration. The most obvious and simple interpretation of them is, that the ratios are those of the respective amounts of *vis inertiæ* possessed by the vibrating atoms which originate the lines; while their arithmetical coincidence with certain musical intervals is merely accidental, and such as might be expected, according to the law of probabilities, where so large a number of lines are concerned.

If diverse rates of vibration, having their origin harmonically in a common rate of vibration, might be looked for anywhere, it is in the lines produced by the same element. Yet such lines are not, as a general rule, thus harmonically related. The principal fixed lines E and G are both iron lines; but there is no harmonic connection between them, although E appears to be harmonically related to another iron line in the indigo, and G to another in the green. But the number of iron lines is so great that these may well be mere arithmetical coincidences. If we take another example, such as magnesium, in which the lines are few and conspicuous, we shall find that their ratios do not correspond to any musical interval. These magnesium lines are four in number, and their wave-lengths, according to Angström's scale, are 5527·54 in the yellow, 5183·10, 5172·16, and 5166·88—all three in the green. The ratios subsisting between any two of these are too small to be harmonic. The ratio between the first and last, though greater than a semitone, is less than a tone. Between the first and third the ratio approaches near to that of *do* to *re*♭ in the Pythagorean scale; but this interval is highly discordant.

On the whole, therefore, whether we take the mean colours of the spectrum, the principal fixed lines, or the lines produced by any single substance, it cannot be affirmed that there is between colours and musical tones any analogy, beyond that of their being both produced by vibrations; while the relations of those vibrations are in each case governed by mathematical laws. But these laws are in the case of colours much more simple and regular than in the case of musical sounds, in which they are discontinuous, irregular, and complex.

The points of diversity between the two sorts of vibrations are also very marked. The normal eye can judge much more promptly and correctly of a simple colour than can

the normal ear of a simple musical note. In colour there is always involved the idea of superficies; and although time is also really involved, yet the rapidity of vibration is such that the mind can form no conception of it whatever. In music, on the other hand, there is not involved any idea of superficies; whereas time is an indispensable element in the conception of a musical note. Again, there is in music nothing corresponding to complementary colours or to the perception of mere whiteness, which can be produced by a combination of such.

Harmony in music is produced by the simultaneous impulse on the ear of two or more combined sounds, whose rates of vibration stand to each other in certain definite arithmetical relations; and whenever there is any departure from those relations the result is either dissonance or discord. When two or more colours fall simultaneously on the same point of the retina, the result is a compound colour, which may or may not be pleasing to the eye; but the mixture of adjacent colours in the spectrum is not displeasing to the eye, as would be the simultaneous sounding of two adjacent musical notes to the ear. What is called harmony in colour depends, not on the simultaneous impulse of two or more waves of colour on one and the same point of the retina, but on the juxtaposition of two or more colours without admixture. The effect seems to depend on the definite arithmetical relations which the rates of vibration corresponding to the colours bear to each other, as in the case of sound.

These effects of the juxtaposition of colours, however, are much more analogous to melody in music than to harmony. The pleasing impression, for example, produced by the gradual blending of the adjacent colours in the refracted spectrum is analogous to the slide in the violin. The juxtaposition of pure orange and pure violet doubtless owes its agreeable impression to the circumstance that these two colours stand to each other in the same ratio as do the *do* and *sol* of the musical scale. In like manner the effect produced by the juxtaposition of pure red and pure green is due to their standing to each other in the same ratio as *do* and *fa*. So also with pure red and pure violet, which bear the same mutual relation as *do* and *la*, or pure green and pure violet, which are related as *do* and *mi*; likewise pure orange and pure green, which are related as *do* and *mi*♭. But in all these cases the effect is more analogous to that produced by the striking of these notes in rapid succession as parts of a melody, than to the harmony resulting from

Table of Spectral Lines and Musical Intervals.

Line.	Sign.	Ideal.	Pythag.	Harmonic.	W. Length.	W. L. Ang.	Difference.	Elements.	Colours.
A.	÷	—	Sol♭	—	5340·54	5339·35	1·19	Fe.	Gr.
	÷	Fa♯	—	—	4790·23	4791·78	1·58	Co. Ti.	Bl.
	÷	—	—	La♭	4752·51	4753·47	0·96	Mn.	,,
	÷	La	—	—	4521·38	4522·09	0·71	Ti.	In.
	÷	Si	—	—	4028·1	4029·5	1·4	Mn.	Vi.
	÷	—	Si	—	4005·4	4004·9	0·5	Fe.	,,
B.	÷	—	—	Re♭	6437·91	6438·35	0·44	Ca.	Or.
	÷	—	Re♭	—	6430·64	6430·12	0·52	Fe.	,,
	÷	—	Sol♭	—	4822·98	4822·9	0·08	Mn.	Bl.
	÷	Sol♯	—	—	4326·0	4325·34	0·66	Fe.	In.
C.	÷	—	Mi♭	—	5462·24	5462·44	0·2	Fe.	Ye.
	÷	Mi	—	—	5208·34	5207·78	0·56	Cr.	Gr.
	÷	—	Mi	—	5184·86	5183·1	1·76	Mg.	,,
	÷	—	—	Sol♭	4666·37	4666·45	0·08	Fe. Ti.	Bl.
	÷	—	Sol♯	—	4152·57	4153·79	1·22	Fe.	Vi.
	÷	Sol♯	—	—	4133·86	4133·94	0·08	Fe.	,,
D.	×	Re♭	—	—	6245·66	6245·62	0·04	Fe.	Or.
	÷	—	—	Re♭	5526·60	5527·54	0·94	Mg.	Ye.
	÷	Mi♭	—	—	4957·28	4956·87	0·41	Fe.	Gr.
	÷	—	Fa♯	—	4196·82	4197·98	1·16	Fe.	Vi.
	÷	—	—	Sol♭	4192·0	4191·17	0·83	Fe.	,,
E.	×	Mi	—	—	6265·54	6264·31	1·23	Fe.	Or.
	×	—	Re♯	—	6244·35	6245·62	1·27	Fe.	,,
	×	Mi♭	—	—	5913·88	5913·3	0·58	Fe.	,,
	÷	—	Re♭	—	4933·8	4933·55	0·25	Fe. Ba.	Gr.
	÷	—	—	Mi♭	4492·83	4493·81	0·98	Fe.	In.
F.	×	—	Re♯	—	5760·89	5762·04	1·15	Fe.	Ye.
	×	Re	—	—	5456·0	5454·84	1·16	Fe.	,,
	×	—	—	Re♭	5184·8	5183·1	1·7	Mg.	Gr.
	÷	—	Re♯	—	4101·26	4101·2	0·06	Hy.	Vi.
G.	×	—	Sol	Sol	6460·85	6461·98	1·13	Ca.	Or.
	×	—	—	Mi♭	5168·68	5168·48	0·2	Ni. Fe.	Gr.
	÷	—	Re♭	—	4033·47	4032·9	0·57	Mn.	Vi.
H.	×	Mi	—	—	4955·27	4956·87	1·6	Fe.	Gr.
	×	—	Re	Re	4424·63	4425·07	0·44	Ca.	In.
	×	Re	—	—	4414·64	4414·77	0·13	Fe. Mn.	,,
	×	—	Re♭	—	4199·95	4201·56	1·61	Fe.	Vi.
	×	—	Do♯	—	4143·4	4143·14	0·26	Fe.	Vi.

their being struck simultaneously, the analogy to which can arise only from the perfect admixture of colours.

The effect produced on the eye by the juxtaposition of complementary colours, again, seems to depend on a different principle from that of the rates of vibration standing to each other in a musical ratio. It results solely from the circumstance that the admixture of the two adjacent colours would give white; and as three colours are required to produce this effect, one or both of the adjacent complementary colours must contain the necessary third colour in the proper proportion required to constitute white. The retina experiences a pleasurable relief on turning from the one complementary colour to the other, because the vibrations are then most opposed. Here, also, the effect is more analogous to melody than harmony. The ear experiences the same weariness from the prolongation of one note as the eye does from gazing on one colour; and as the latter feels the greatest amount of relief when turned to the complementary colour, so the ear feels the greatest amount of relief, when, after being fatigued with one note, it hears another which would make with it a harmonious combination, as the third, fifth, or octave.

It is, however, in their metaphysical qualities that the two sorts of vibrations most widely differ; and here the advantage rests with the musical tones. Apart from variety of form, colours can be regarded only as more or less pleasing or the reverse. Beyond this they have little or no emotional power. Music, on the other hand, addressing the imagination, can express, awaken, or exalt every emotion of the mind. It is only when united to variety of form that colours acquire the ascendency over their sonorous rivals. Then, indeed, they become by much the more powerful vehicle for conveying ideas, whether intellectual or emotional, to all but the blind.

VI. REMARKS UPON THE PRESENT STATE OF THE DEVONIAN QUESTION.

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ONE of the most interesting questions that has of late years perplexed the minds of geologists, and one which we might almost say has been a vexed point ever since the district was studied, is the age and relations of the slaty rocks and limestones of West Somerset, Devon, and Cornwall. Originally called "Greywacke" and transition slates, the beds below the culm-measures or true coal-measures were subsequently called "Devonian," and regarded as the marine equivalent of the old red sandstone. Latterly this classification has been called into question, and it has been urged that the greater part of the Devonian rocks are of lower carboniferous age. This last opinion being a matter of great dispute, it may be interesting to review the present state of the question.

Upon glancing at a geological map of the country, such as Greenough's, we find that part of Devon north of Barnstaple and South Molton, and that part of West Somerset which includes Exmoor Forest, the Brendon and Quantock Hills, to be coloured a uniform tint as Devonian, corresponding to that of the old red sandstone of the Mendips, South Wales, and Herefordshire. This area of the Devonian rocks is bounded on the south by the culm-measures or carbonaceous series, and the boundary line between the two formations is marked on either side by narrow and apparently impersistent bands of limestone, which, to judge from the map alone, would appear to bind them together in conformability.

Until the late Mr. Jukes brought forward his views upon the subject, the age of these formations was generally looked upon in this way—that the Devonian rocks represented in time the old red sandstone, and that the culm-measures were of carboniferous age, newer than the mountain limestone. In Greenough's map these latter were coloured as the representation of the millstone grit, and the same is the case in Ramsay's geological map of England and Wales.

This apparent indecision as to the true age of the culm-measures has necessarily caused much obscurity when the relations of the two series, and their generally acknowledged conformability, have been taken into consideration.

Looking to the origin of the term "Devonian," we learn that Mr. Lonsdale was the first to point out that the characters of the fossils of the Devon limestones seemed to give them an intermediate place between the upper silurian rocks and the mountain limestone, and it was this suggestion which in 1839 led Sedgwick and Murchison* to adopt the term "Devonian system" for the series of rocks in North and South Devon which underlie the culm-measures. Henceforth they were regarded as contemporaneous with the old red sandstone. It was even then hinted that possibly the mountain limestone was represented by a part of the culm-measures, and when one refers to the subsequent papers one sees how much room there was to doubt the clearness of this correlation, and in the writings of De la Beche particularly, we find the difficulties attending it fully pointed out.† This is apparent when he compares the Upper Devonian rocks with the upper portion of the old red sandstone, as exhibited at no very great distance apart. For the upper beds of the old red sandstone in South Wales and the Mendip Hills show no similarity whatever to the Upper Devonian rocks. He, moreover, refers to the view taken by Mr. (now Sir Richard) Griffith in 1842, who then pointed out the strong resemblance between the North Devon rocks and those beds in Ireland, which he called carboniferous slate and yellow sandstone, deposits equivalent to the transition beds, or lower limestone shale, between the old red sandstone and the mountain limestone.‡ These views, in fact, were almost the same as those at which Mr. Jukes arrived.§ It was in 1866 that his famous paper was read before the Geological Society of London,|| and therein Mr. Jukes brought forward (though not for the first time) the views, which for fifteen years previously he had been thinking over, and which led him to consider the rocks of North Devon to belong partly to the group called carboniferous slate in Ireland, and partly to the old red sandstone. He based his interpretation upon an intimate knowledge of the geology of the South of Ireland, where he found that the mountain limestone which was separated from the old red sandstone by the carboniferous slate became in places entirely replaced by the slate, so that this slate then filled

* Trans. Geol. Soc., 2nd Ser., vol. v., p. 633.
† Mem. Geol. Survey, vol. i., p. 65.
‡ *Idem*, p. 76.
§ Additional Notes on the Grouping of the Rocks of North Devon and West Somerset. 8vo. Dublin, 1867. P. 19.
|| Quart. Journ. Geol. Soc., vol. xxii., p. 320.

up the whole of the interval between the top of the old red sandstone and the base of the coal-measures. Here he contended was the clue to determine the structure of North Devon; the order of sequence appeared to him the same in both localities, so that the so-called Devonian rocks were really the lower portion of the carboniferous system, resting, as in Ireland, upon a base of the old red sandstone.*

In 1867 Mr. Jukes published a small map, which more clearly expressed his ideas. The true old red sandstone he considered to occur at the North Foreland, Minehead, and Croydon Hill, also at the north-western end of the Quantock Hills; then succeeded the carboniferous slate at Lynton, Combe-Martin, Ilfracombe, Mortehoe, and the Brendon Hills; while stretching from Pickwell Down to Haddon Down, Mr. Jukes identified another band of old red sandstone, to account for which he considered that a great fault, with a downthrow to the north, occurred along this line and repeated the beds to the south,—the carboniferous slate coming conformably over this band of old red sandstone, and then again passing gradually into the culm-measures above.

Mr. Jukes's views met with great opposition at the time, but as few, if any, of his opponents had a personal knowledge of the geology of the South of Ireland, they could not perhaps fully realise all the facts which guided him in his inferences.

Mr. Etheridge,† however, took up the question in great detail, and though perhaps he laid greatest stress upon the palæontological evidence, he yet disputed the conclusions of Mr. Jukes on physical and stratigraphical grounds, and maintained that there was no evidence of any fault, as the succession of the strata and the groups of associated fossils from the North Foreland to Barnstaple was continuous and natural. The area he considered to be occupied by three well-defined groups—the Upper, Middle, and Lower Devonian, chronologically equivalent to the whole of the old red sandstone, but deposited under different mineral and life conditions, and in a *different geographical area.* The fossil evidence, in his opinion, was against any repetition of the beds, and nowhere justified the proposition that the Devonian beds were synchronous with the carboniferous. In discussing this question, however, Mr. Jukes argued that the difference between the fossils from different parts of the so-called Devonian rocks did not differ more markedly from

* *Vide* JUKES and GEIKIE, Manual of Geology, 1872, p. 762.
† Quart. Journ. Geol. Soc., vol. xxiii., p. 568.

each other than fossils from different parts of the carboniferous slate differed from each other; that the fossils of both groups warranted the conclusion that they might have been geologically contemporaneous.*

The palæontological evidence cannot, therefore, be looked upon as decisive. The Devonian beds contain some species which are also found in silurian rocks, and many species that occur in the mountain limestone. The old red sandstone does not contain any of these fossils; "there are *no* marine forms in the old red sandstone."† Certain fish remains have, however, been found by Mr. Pengelly‡ in the Devonian rocks of Cornwall, which are also found in the old red sandstone. These include *Pteraspis* and *Phyllolepis concentricus*. This, Mr. Jukes remarks, is the strongest presumptive evidence yet derived from fossils in favour of the contemporaneity of the two formations. Nevertheless, he adds, it is not conclusive proof, for it is obvious that the occurrence in the Devonian rocks of species of fossil fish belonging to the same genera as those of the old red sandstone no more proves the Devonian beds to have been contemporaneous with the old red sandstone, than the occurrence of species of trilobites, of the same genera as those in the Silurian rocks, prove the Devonian rocks to be contemporaneous with the Silurian.§ Therefore, we must agree with Jukes that the geological age of the fossils must be proved by the stratigraphical position of the beds.

In setting forth the present state of the question, two points connected with the subject, which have recently been brought forward, may be referred to.

An interesting feature has been noticed by Mr. T. M. Hall in connection with the granites of Lundy Island, South Devon, and Hestercombe, near Taunton (first described by Mr. Leonard Horner‖). He remarks that, although the most remote of the three patches, "the so-called granite (syenite) of this last locality has been regarded as possessing a more intimate connection with Lundy Island, since the general run of the Palæozoic rocks in North Devon and the adjoining portion of West Somerset is from east to west; and it might, therefore, be suggested that least resistance would be afforded to the intrusion of an igneous rock

* Jukes and Geikie, Manual of Geology, 1872, p. 763.

† Etheridge. *op. cit.*, p. 679.

‡ Mr. Pengelly stated that he had found 300 specimens of Pteraspidian fishes in the Devonian rocks. Brit. Assoc. Meeting, Exeter, 1869.

§ Jukes, Notes on Parts of South Devon and Cornwall, p. 42.

‖ Trans. Geol. Soc., vol. iii., p. 348.

at the various places situated along the same line of strike."*

The chief point of interest connected with these observations is the bearing they may have on the supposed fault of Mr. Jukes, a consideration which in reference to the little syenitic dyke at Hestercombe was suggested by Mr. Bristow, when on a visit to this spot on geological survey work.

The age of the Cannington Park limestone has been a constant source of discussion, and still the opinions vary. Mr. Etheridge† spoke in decided terms of its Devonian characters, and of its dissimilarity to the mountain limestone; while more recently its identity with this latter formation has been again insisted upon.‡ The scarcity of fossils has somewhat hindered its true position being established: but Mr. S. G. Perceval§ has lately examined a series of corals collected there, and which he finds to be of true carboniferous genera and species. The structure of the limestone he also identifies with that of the mountain limestone of the neighbourhood of Bristol. The very diversity of opinion on this patch of limestone would seem to mark it as a connecting link between the mountain and Devonian limestones, and so to lend support to Jukes's view that both belong to the same period.

Thirty years ago De la Beche remarked, that "from the increased knowledge we have lately had of the beds which may be considered as the passage of the old red sandstone into the carboniferous limestone, as well in Ireland as in South Wales, and in adjacent parts of England, we have endeavoured to point out, as not improbable, that in North Devon some part, at least, of the accumulations there exposed might be referable to that date." He also observes that "there is much leading us to infer that in South Devon the accumulations under notice were not far removed from a similar geological date."‖ Mr. Etheridge admits that there may be grounds for endeavouring to establish contemporaneity between the Upper Devonian series of North Devon and the carboniferous slates of the South of Ireland.¶

* T. M. Hall, Notes on the Geology and Mineralogy of the Island of Lundy. Trans. Devon Assoc., 1871.

† Quart. Journ. Geol. Sec., vol. xxiii., p. 581.

‡ H. W. Bristow and H. B. W., Geol. Mag., vol. viii., p. 504.

§ Geol. Mag., vol. ix., 1872, p. 94.

‖ Mem. Geol. Survey, vol. i., p. 97.

¶ *Op. cit.* p. 690.

The precise age of the culm-measures and their relations to the Devonian rocks are points which at first strike one as of great importance.

The perfect conformability of the northern boundary of the culm-measures with the Devonian rocks has generally been admitted, by Sedgwick and Murchison, and most other geologists.

In South Devon, however, an unconformability has been pointed between these formations. This was described by Mr. Godwin-Austen, and latterly by Dr. Holl, who remarks that the base of the lower culm-measures does not everywhere rest on the same part of the underlying Devonian rocks. He adds, that "this unconformability on the southern side of the culm-trough is so considerable that it throws doubt upon the reality of the apparent regular succession to the north, and leads to the suspicion that the conformability which is there supposed to exist may be more apparent than real." *

Mr. Jukes,† however, points out that there is really no proof of this unconformability in South Devon, owing to the difficulty in deciding between stratification and cleavage, and the many disturbances to which the beds have been subjected.

Mr. T. M. Hall, remarking on the Devonian and culm-measures, says—"The two great systems pass quite insensibly one into the other, without any distinct line of separation between them."‡ And this is evident from the sections exposed in quarries and in the cuttings of the new railway between Barnstaple and Taunton, for one passes from one series to the other before one is aware of it; there is no sudden break or change.

The age of the culm-measures is now admitted to be that of our true coal-measures. For in the evidence given before the Royal Coal Commission there was some question as to whether the coal-measures likely to be found to the south of the Mendips might not be of the type of the Devonian culm-measures; and Mr. Etheridge also said that he was inclined to think that the Devonshire coal-field was part and parcel of the South Wales coal-field, the lowest portion of it, but deposited under very different conditions,—an opinion which was indeed arrived at by Sedgwick and Murchison. He thought that the impure coals of the

* Quart. Journ. Geol. Soc., vol. xxiv., p. 442.

† Notes on Parts of South Devon and Cornwall.

‡ Geology of Lundy Island.

Millstone Grit series were the equivalents of those beds which lie south of Barnstaple.*

Mr. Jukes identified the culm-measures as exactly like the Irish coal-measures, especially in the Kilkenny coal-field.†

From this it naturally follows that the beds beneath the culm-measures must represent the lower carboniferous rocks, and in part, at any rate, Mr. Jukes's notions must be correct. He would limit the term Devonian, and retain its use, for those beds containing the marine fossils commonly known under the name of Devonian fossils. The old red sandstone does not contain any of these fossils, and is a group of rocks distinct and altogether below them. He further ventured to advance the notion that the Devonian beds may rather be looked upon as the most general type of those which intervene between the coal-measures and the old red sandstone, and that the mountain limestone is rather a local and exceptional peculiarity.‡

On the other hand, Mr. Etheridge considers that we must either admit that the Devonian is a marine equivalent in time of the old red sandstone, or that it must be a distinct life-system, occupying an immense area, spreading over an enormous interval of time between the completion of the old red sandstone as a whole and the commencement of the succeeding and well-marked carboniferous series.§ The latter opinion seems to be that generally adopted; for in remarking upon the opinions since expressed, if we do not find a tendency towards the acceptance of Mr. Jukes's views, we see that geologists are beginning to regard the Devonian rocks as newer than the old red sandstone.

Mr. Godwin-Austen has stated that he had always regarded the Devonian system as merely an older member of the Carboniferous, holding much the same relation to it as the Neocomian to the Cretaceous; and that he would be glad to see it recognised, not as an independent system, but merely as the introduction of that far more important system the Carboniferous, during the deposit of both of which the globe presented the same physiographical conditions.‖

Professor Phillips, too, observes that "the old red sandstone is followed in Devonshire, and still more remarkably

* Report of Coal Commission, vol. ii., p. 421.
† Notes, &c., p. 31.
‡ Quart. Journ. Geol. Soc., vol. xxii., p. 369.
§ *Ibid.*, vol. xxiii., p. 613.
‖ *Ibid.*, vol. xxviii., 1872, p. 30.

in the South of Ireland, by a series of shales, grits, and limestones, with a large *suite* of fossils, having on the whole a considerable analogy with the still richer associations of marine life in the carboniferous limestone. . . . Near Linton, in North Devon, and south of Plymouth, we may satisfy ourselves of the fact that old red sandstone underlies the Devonian beds. . . . From this series of rocks to the carboniferous strata which succeed the transition is easy,—so easy indeed that, in the opinion of Sir R. Griffith and Mr. Jukes, the whole of the Devonian series may be united with the lowest members of the Irish carboniferous group (yellow sandstone and carboniferous slate). What seems ascertained truth is the close approximation in time, in character of deposition, and in forms of life, of the South Hibernian and South Welsh rocks; while the North Devonian strata contain with these a somewhat lower group, not distinctly represented in Wales or Ireland."*

Whether we regard the Devonian slates as the equivalents of the old red sandstone or of the lower carboniferous rocks, a great change in sedimentary condition must have taken place; and the question is still perhaps to be decided, whether part of the Devonian rocks are a modified extension of the old red sandstone—a point which appears to take its stand merely on palæontological evidence—or whether the whole of the fossiliferous Devonian slates and limestones be not of lower carboniferous age, the representatives of the mountain limestone and the lower limestone shale, and of the carboniferous slate and limestone of Ireland. This latter opinion finds the more support when we look, as Mr. Jukes and others have pointed out, to the variations which take place in the carboniferous limestone series when traced through the north of England into Scotland, as well as through the South of Ireland.

Looking at the culm-measures as representing the true coal-measures, and perhaps also the millstone grit, and that they pass gradually downwards into the Devonian rocks, we may possibly find, in the numerous thin bands of limestone which occur along the junction, some feeble representation of the upper part of the mountain limestone; then come a series of slates, which must in part represent the mountain limestone, the whole of the lower limestone shale, carboniferous slate, and perhaps a part of the old red sandstone. Beneath these come beds of the acknowledged type of the old red sandstone.

* Geology of Oxford and the Valley of the Thames, p. 79.

At any rate, in this conformable series we have to look for the equivalents of the lower carboniferous series. It may not be possible to fix any of the divisions, as we find them marked at no very great distance away, in South Wales and in the Mendip Hills; but in these places it is often difficult enough to fix a precise line, so gradually do they merge one into the other, though they are clear enough when looked at in a large way. A greater similarity of conditions prevailed over the Devonian area, and naturally the fossils differ from those found elsewhere in varying sedimentary deposits of the same period.

Whether the supposed fault of Prof. Jukes can be proved or not is a matter that it is difficult to foresee. Possibly the new line of railway in course of construction between Barnstaple and Ilfracombe may afford some decisive evidence. Let us hope, at any rate, that it may yield many good sections. At present, as Mr. Jukes remarks, whilst there is no direct evidence of the fault, yet no certain physical or stratigraphical evidence has been adduced against it.

That there is much to be done in this field is a point about which no doubt can exist. The workers have been many; and the names of Sedgwick, Murchison, Lonsdale, De la Beche, Godwin-Austen, Phillips, Jukes, and Etheridge, must always command the highest respect of the followers in the same field.* They have all done great work in elucidating the structure of a difficult country; and as their followers have the advantage of their labours, so the path becomes easier, and whenever a final solution of the question is arrived at, it will probably be by a transition in opinion as easy as that which binds the series of rocks together.

* A list of works on the Geology of Devonshire has been compiled by Mr. Whitaker. See Trans. Devon. Assoc., vol. iv., p. 330, and vol. v., p. 404.

NOTICES OF BOOKS.

The Expression of the Emotions in Man and Animals. By CHARLES DARWIN, M.A., F.R.S., &c. London: Murray. 1872.

AN insatiable longing to discover the causes of the varied and complex phenomena presented by living things seems to be the prominent characteristic of Mr. Darwin's mind. Nothing is so insignificant as to escape his notice or so common as not to demand of him an explanation. The restless curiosity of the child to know the "what for," the "why," and the "how" of everything (a wholesome curiosity which our educational system represses, and which rarely survives to manhood) seems with him never to have abated its force; but he is by no means satisfied, as the child is, with mere verbal explanations which really explain nothing, or, as many writers on this particular subject have been, with purely speculative explanations which are wholly unsupported by evidence.

The present work exhibits these characteristics of the author's mind in an eminent degree, since we here find systematised and explained by means of acknowledged physiological and psychological facts all the immense variety of complex movements and minute muscular contractions, by the observation of which we unconsciously interpret, with more or less certainty, the almost infinitely varied passions and emotions of men and animals. How few of us have ever thought of asking for a reason why infants shut their eyes tightly while screaming; why we shrug our shoulders or stand erect, blush or grow pale under different emotions; why a dog crouches and a cat arches its back when affectionate; or have even imagined that satisfactory reasons for these things could be given? Yet we can hardly help being interested in so novel an enquiry, and one which throws so much light on actions and movements which constitute a kind of universal language, but which have hitherto appeared arbitrary and inexplicable to us.

The result of Mr. Darwin's study of this subject is the establishment of three general principles, which explain and give a meaning to almost all those involuntary gestures and movements by which men and animals express their emotions. The first of these principles is that of Serviceable Associated Habits. When any action has been useful or necessary under a certain state of mind, it will from association continue to be performed whenever the same state of mind recurs, even if of no use. As an instance we may take the case of dogs turning round several times before they lay down to sleep even on a carpet or floor, and sometimes giving a few scratches, a practice which was no doubt useful when the wild animal slept among herbage out of doors, and which

is continued now as a habit when of no such use. The second is the principle of Antithesis, which is, that certain actions or attitudes being the natural acompaniment of a given emotion or state of mind, the opposite state of mind will be expressed by actions or attitudes which are, as far as possible, the exact opposites of the former. A good example of this is given by the case of the dog and cat. The former crouches down and holds down its tail when licking its master's hands or jumping on his knees; but the cat while rubbing against its master's leg, stands erect with somewhat arched back and tail up on end. These attitudes are explained by their being in each case the opposite of those assumed when the animals prepare to fight. The dog stands erect, holds up his tail and bristles up the hairs on his back and shoulders; the cat crouches down with paws out and the tail laid flat on the ground, and gently waved from side to side. When the opposite emotions of gentleness, submission, and affection occur, the attitudes assumed are as remote as possible from those associated with anger and pugnacity.

The third principle is, that certain actions expressive of certain states of mind are the direct results of the constitution of the nervous system, being almost wholly independent of the will and of habit. Trembling under the influence of fear, or rage, or joy, is an example of this. It is of no use and it is quite involuntary; it cannot, therefore, have been acquired by the means already pointed out. It may be said that this is merely a confession of ignorance, and so it is in some cases; but in others Mr. Darwin traces the causes in the known action of certain nerves or muscles, and so gives a valid explanation. Such is the case with the firm closure of the eyes by screaming infants. This is quite involuntary, and does not occur later in life, but the whole mechanism by which it is produced has been traced out, and it is found that it is a provision to prevent injury to the delicate vessels of the eyes by the increased flow of blood to the head during violent screaming.

By means of a series of questions sent to correspondents in various parts of the world, Mr. Darwin has ascertained that many well-known modes of expression are almost universal. Even such an apparently conventional action as the shrug of helplessness or apologetic refusal has been observed among various savage races. Being thus proved to be a natural, not an acquired, expression, it becomes necessary to account for it, and this is done on the principle of antithesis; every part of the expression being the opposite of that which implies determination and action. Comparatively few human expressions, on the other hand, can be distinctly recognised in animals, that of sneering by raising the upper lip on one side, and thus showing the canine teeth, being one of the most curious. There is a very elaborate discussion on blushing. This is a peculiarly human attribute, being observed in almost every race of man, but not in the lower

animals. It has been thought by some to be a special endowment for the purpose of expressing modesty or shame, but Mr. Darwin objects to this view, because it occurs in dark races, when it is hardly visible, and also because shyness is the most frequent cause of blushing, and this is of no use, and makes both the actor and spectator equally uncomfortable. The theory adopted is, that blushing is caused by self-consciousness directed chiefly to our personal appearance, and is therefore generally exhibited in the face, to which attention is most directed, and the skin of which is very sensitive. Much evidence is adduced to show that attention directed to any part or organ can affect its condition or action, and this is the physiological fact on which the explanation rests. Great confusion of mind often accompanies blushing, and is supposed to be caused by it. But it seems more probable that it is caused by the whole attention being so powerfully directed to ourselves as to interfere with the action of the mind in any other direction. A remarkable instance of this confusion is given by Mr. Darwin on the authority of an eyewitness:—

"A small dinner party was given in honour of an extremely shy man, who, when he rose to return thanks, rehearsed the speech, which he had evidently learnt by heart, in absolute silence, and did not utter a single word; but he acted as if he were speaking with much emphasis. His friends perceiving how the case stood, loudly applauded the imaginary bursts of eloquence whenever his gestures indicated a pause; and the man never discovered that he had remained the whole time completely silent. On the contrary, he afterwards remarked to my friend with much satisfaction that he thought he had succeeded uncommonly well."

It has been an objection to Mr. Darwin's theory of the "Origin of Species," that the rattlesnake warns its prey of its vicinity, and that such a habit could not possibly have been acquired by natural selection. In a very interesting discussion on the means of exciting fear in an enemy, Mr. Darwin gives a fuller statement of his views on this subject than he has done in any of his former works. He finds that various kinds of reptiles inflate themselves, hiss, open their mouths, and assume a ferocious aspect as a means of protection against attack. The cobra dilates its hood when alarmed or excited, and the puff adder swells and hisses with a sound hardly distinguishable from the rattle of the rattlesnake. He believes, therefore, that all these various sounds and appearances are warnings to would-be devourers that the creatures who produce them are dangerous. The rattle of the rattlesnake is said to imitate closely the sound of a cicada inhabiting the same region, and it has been supposed that it serves the purpose of attracting insect-eating birds as the snake's prey; but this view is rendered improbable by the fact that the snake rattles when alarmed or threatened. If it is proved to be a warning to

enemies, it becomes useful to the creature itself, and could, therefore, have been acquired by natural selection.

In some cases the explanations given seem far-fetched, or simpler ones appear to be overlooked. I can hardly believe that when a cat, lying on a shawl or other soft material, pats or pounds it with its feet, or sometimes sucks a piece of it, it is the persistence of the habit of pressing the mammary glands and sucking during kittenhood; nor that the frequent practice of cats rubbing against their master's legs is derived from the habit of fondling their young. The habits and ideas of infancy seem to be completely lost in adult life, and to be replaced by others widely different; and it seems hardly likely that they should persist so strongly in one or two isolated instances without leaving more frequent and less equivocal traces behind them.

When a horse breaks into a gallop, at full speed, he always lowers his tail, and this is said to be done in order that as little resistance as possible may be offered to the air. This reason seems very fanciful, when the obvious explanation occurs, that, as the whole available nervous energy is being expended in locomotion, all special muscular contractions not aiding in the motion cease. It also seems very unsatisfactory to refer the vague and undefined yet deep emotions often excited by music to a recalling or survival of "strong emotions felt during long past ages, when, as is probable, our early progenitors courted each other by the aid of vocal tones," although it is very difficult to suggest any other explanation.

The open mouth, and raised arms with open hands turned outwards, is an expression of astonishment very general all over the world. Mr. Darwin explains the open mouth by a complication of causes, but he omits to notice, what seems to me a very probable one, that it represents an incipient cry of alarm or fear, or call for help. The raising of the arms and the open hands are explained by antithesis, they being the opposite of a state of indifference or listlessness. But this seems very unsatisfactory. The attitude is too definite, too uniform, and too wide-spread, to be derived from such a vague and variable cause as the opposite of a position of unconcernedness. There seems, however, to be a very obvious and natural explanation of the gesture. Astonishment, among our savage ancestors, would most frequently be excited by the sudden appearance of enemies or wild beasts, or by seeing a friend or a child in imminent danger. The appropriate movement, either to defend the observer's face or body, or to prepare to give assistance to the person in danger, is to raise the arms and open the hands, at the same time opening the mouth to utter a cry of alarm or encouragement. It is the protective attitude of an unarmed man to be ready to ward off attack of some uncertain or undefined kind; and very nearly the same attitude is that which we adopt as we rush to the assistance of some one in danger, our hands

ready to grasp and save him. When used by us as a mere sign of astonishment, at some strange but harmless phenomenon, it has become to a great extent conventional, but the origin here advocated is rendered probable by a remark of Mr. Darwin himself, that, as one of the expressions of fear, "the arms may be protruded as if to avert some dreadful danger;" and among savages almost every source of astonishment would excite more or less fear.

It is rather curious that an author who is not usually satisfied with anything less than a real and intelligible explanation, should yet be so ready, in some cases, to admit innate ideas or feelings. Among the numerous, and often most interesting, observations on his own children, Mr. Darwin tells us that a child six months old was distressed at seeing its nurse pretend to cry. He thinks, in this case, that "an innate feeling must have told him that the pretended crying of his nurse expressed grief; and this, through the instinct of sympathy, excited grief in him." Now, although I imagined myself much more disposed to believe in innate ideas than Mr. Darwin, I cannot see the necessity for them here. A child at that age often cries or is distressed at any strange face, or even at the sight of a friend in a strange dress. The nurse's attitude and expression were strange; they made her look unlike herself, and the child got afraid, and was about to cry. That seems to me a better explanation than that the child had an innate knowledge that the nurse was grieved.

Somewhat akin to this is a readiness to accept the most marvellous conclusions or interpretations of physiologists on what seem very insufficient grounds. In discussing the subject of reflex action Mr. Darwin quotes the well-known experiment of the decapitated frog, which is said to wipe off a drop of acid from its thigh by a motion of the foot of the same leg. But if this foot is cut off it makes several fruitless efforts, then stops a while, as if restless and seeking some other way, and then, *by using the other foot, succeeds in wiping off the drop of acid.* Now this is imputed to pure reflex action, and not a word of doubt is thrown either on the experiment or on the inference from it. Yet it seems to me absolutely certain, either that the experiment is not correctly recorded, or that, if correct, it demonstrates volition and not reflex action. For surely reflex action cannot produce, in a decapitated frog, movements which were probably never once performed by the living frog. The action of drawing up the leg in swimming or leaping is one which the frog performs incessantly during its whole life; it would therefore probably be performed under any suitable stimulus by reflex action, and might, as a consequence of the usual motions, wipe off the drop of acid from a place which the foot, during contraction, would naturally reach. But the action of crossing one foot over to the thigh of the other leg is one which was very rarely, if ever, performed, because during life the frog possessed

both its feet. Again, reflex action cannot be set up without a suitable stimulus. The stimulus applied to one leg set up reflex action in that leg, or perhaps, by co-ordination, of the muscular movements in the two legs; but, when one foot was cut off, what caused the nature of the motion to change, and a new set of muscles to be called into action, with such precision as to apply the foot to an unaccustomed part of the body? This is the work of consciousness; first to *know* that the one motion failed to produce an effect aimed at, next to change the motion so as to produce the *desired* effect. The experiment is described as if all this were really done by reflex action; but, if so, then what need have we of consciousness in animals at all, and why may not all their motions and actions during life be so produced? If the experiment, as recorded, is strictly accurate, it appears to me to demonstrate consciousness and volition, on the part of the frog, without a brain,—a fact by no means incredible in itself, but one which, if established, might have important consequences.

The book is admirably illustrated, both by woodcuts and by a number of photographs representing the most characteristic expressions. It is written with all the author's usual clearness and precision; and although some parts are a little tedious, from the amount of minute detail required, there is throughout so much of acute observation and amusing anecdote as to render it perhaps more attractive to general readers than any of Mr. Darwin's previous works.

ALFRED R. WALLACE.

The Hygiene of Air and Water: being a Popular Account of the Effects of the Impurities of Air and Water, their Detection, and the Modes of Remedying them. By WILLIAM PROCTER, M.D., F.C.S., Surgeon to the York Dispensary, and formerly Lecturer on Chemistry and Forensic Medicine in the York School of Medicine. London: R. Hardwicke. York: Sampson, Pickering, Johnson, and Tesseyman. 1872. 79 pp.

THE Science of Health in these days is making great advance, and asserts increasing claims for recognition. Its position is a difficult one, for whilst it of necessity lays under contribution the latest discoveries and most abstruse doctrines of modern thought, it must be translated for the comprehension of the bulk of people of the world who have themselves to carry out the precepts which it inculcates. Unfortunately the efforts of the interpreters between Science and the Public are not always successful, and frothy phrases often constitute a large part of so-called popular manuals,—there is a minute morsel of bread to a prodigious quantity of sack. It is a relief to turn to Dr. Procter's little book, which seems to give us exactly what we want; it is

correct in data, terse, practical, and smooth in diction. The author takes a very modest position. He says:—"As treated in the following pages the subject admits of no originality, and the author claims none; his object has been to deal with it in as simple and popular a manner as possible, and to point out the injurious effects produced on health by impure air and water, the sources and origin of their impurities, with the means for their detection, and the several methods by which they may be removed or remedied." Taking the first two pages as a test of the amount of information conveyed, we find, in the course of a brief discussion of the causes of atmospheric impurity, a statement of the normal constituents of atmospheric air, their properties and quantitative relation, the preparation and uses of ozone and the method of testing for its presence, and a word or two concerning the suspended impurities of air. The work goes on to consider the causes which render air impure, the effects of respiration, putrefactive emanations, sewer gas (with useful hints for remedying it), the methods of detecting organic impurity in air, the natural laws for the purification of the atmosphere, ventilation, disinfection, and the hygiene of the sick room.

The second part of the book deals with the impurities of water and their removal. The causes and effects of water contamination, and the relation of typhoid fever and cholera to impure water (a subject on which people will find it greatly to their interest to be enlightened), are well, but briefly, discussed. The description of methods of detecting the impurities of water is succinct, but yet up to the time. Dr. Burdon Sanderson's "zymotic test" is noted, and the methods of detection of nitrogenous matter are simply put. The little volume concludes with hints on the removal of impurities from water.

We recommend it to all; to those whose scientific labours, directly or indirectly, tend to advance or to apply the knowledge of hygiene,—they will find it a useful compendium; to all others whose occupation is in other grooves, but who nevertheless have a personal interest in the preservation of health,—they will find it an easily intelligible and most valuable guide.

A Manual of Microscopic Mounting; with Notes on the Collection and Examination of Objects. By JOHN H. MARTIN, Author of "Microscopic Objects," &c. Illustrations drawn by the Author. 200 pp., 8vo. 11 plates. London: J. and A. Churchill.

THE subject-matter of this volume is divided into seven chapters and an appendix.

The first treats on various apparatus employed; in many instances directions are given for construction, and some of the author's own contrivances are described. Chapters 2, 3, 4,

describe practically the methods of mounting objects dry, in balsam and solution of damar, and in fluids. The system of selecting a list of typical and easily procured objects is a good one: each object so selected is treated separately, and by following out the processes described in these chapters the student who is deprived of the help of more experienced workers will be able to make considerable progress. The author is evidently too fond of the old method of potass maceration in making preparations of insects. The flea as prepared by him is the mere empty skin so common in cabinets: this has only to be compared with specimens mounted in glycerine, without compression, with the contents of the body *in situ*, to cause it to be abandoned, excepting in those cases where the chitinous tissues alone are required. The proboscis of the blow-fly, again, is so treated as to produce the common preparation of the shops,—a mode of mounting which has for years only served to prevent a true knowledge of the structure of this wonderfully complex organ from being obtained. The author has surely never seen some of the insect preparations *au naturel*, which are now far from uncommon in the cabinets of some of our best microscopists.

A great deal of useful information is contained in chapter 5, giving a general summary of various modes of mounting. A large number of interesting objects are here described, and directions given for their examination.

The chapter on Collection gives a great many hints for capturing the small game so much sought after by the microscopist.

Some notice is taken of the important subject of adulterations, but the treatment is so brief—giving little more than a catalogue of adulterating substances—that the information will prove of but little use to the reader. At page 167 the author gives a figure of "a precipitating cell" of his own contrivance, but has unfortunately left out all description, so that the reader is left to make out what he can from the woodcut.

The appendix is one of the most useful portions of the book, containing no less than seventy-seven formulæ for various cements, mounting media, reagents, &c. This microscopic pharmacopœia, compiled from various sources, supplies a real want, and will be duly appreciated.

With regard to the illustrations, the author has certainly not improved in his lithography since the issue of his work on "Microscopic Objects." This is much to be regretted, as Plate 11—a reproduction of some of the author's drawings by the photo-lithographic process—shows that the defect is a want of skill in the manipulation of the lithographic materials. The other plates are characterised by a general coarseness of execution. The figure of flax, Pl. 10, Fig. 92, is unlike any fibre known to the histologist, and the whole plate is a specimen of very coarse wood-engraving. It is a pity that the book should have been spoiled by the bad execution of so important a portion.

With so many admirable existing manuals the present work was scarcely needed: it would have been better if the small amount of new matter had found its way into the pages of one of the periodicals devoted to microscopical subjects.

Records of the Rocks; or Notes on the Geology, Natural History, and Antiquities of North and South Wales, Devon, and Cornwall. By Rev. W. S. Symonds, F.G.S., Rector of Pendock. With numerous Illustrations. London: John Murray. 1872.

No one need be afraid that he will be led into any discussion of the attitude of either science in general or geology in particular in reference to the Bible or ordinary religious teaching. The title, so similar to Hugh Miller's "Testimony of the Rocks," and the clerical position of the author, might lead to this supposition. But not a word of the kind is to be found in the book; in fact the latter part of the title is really a fair exposition of its contents. Mr. Symonds evidently knows his country well, has walked it over and over again, has studied Sir Roderick Murchison's "Silurian System and Siluria" thoroughly, and has given the world the results of his observations. The geology naturally is the principal part of the work, and the order of the work follows that of the Rocks, beginning with the Laurentian, and ending with the Permian. A devout adherent of Sir Roderick Murchison, the author not only follows him over the same ground, but he adopts his theories entirely, and owes very many of his woodcuts to him. The remaining illustrations, mostly by Sir Wm. Guise, are well and carefully drawn. The natural history portion of the work consists mainly of a record of the habitats of rather rare plants, and the resort of various fish; whilst the antiquarian part of the work is the weakest of all, being merely the accounts such as might be found in ordinary guide books of old castles, with an occasional quotation from an ancient chronicler of a passage, the critical authority of which is not very minutely examined. Altogether the book will be found useful by those who are going over the country described, for whilst it is more portable, it also contains more minute detail than "Siluria," and touches upon subjects not alluded to in the other, in all respects, greater work.

A Budget of Paradoxes. By Augustus De Morgan, F.R.A.S., and C.P.S. of Trinity College, Cambridge (Reprinted with the Author's Additions from the "Athenæum"). London: Longmans. 1872.

Many of our readers as they peruse the title of this book will recall with regret a quaint little figure, usually attired in a broad-

tailed dress coat, with an old-fashioned white tie of prodigious dimensions, round spectacles well stopped out with thick black rims, and a small mouth looking very grave, but with a pucker about the corners that betrayed a volcano of fun beneath ever ready to *erupt*. Such was the Budgeteer of Paradoxes. A shrewd thinker, as deep both as a logician and as a mathematician as any of his contemporaries (and he reckoned among his friends Airy, Babbage, Sir John Herschel, and Whewell), he had a fund of humour, and good humour, that one could scarcely have thought could have expended itself on exact science; hence, we may say, arose this collection of inexact science, falsely so called, brought together for the warning and encouragement of future enquirers, and for the amusement of lookers on.

The word "paradox" as used in this book is explained to mean "something which is apart from general opinion either in subject matter or conclusion;" consequently mixed up with the most good humoured banter at circle squarers, trisectors of angles, duplicators of the square, maintainers of the non-rotation of the moon, deniers of gravitation, the rotation and spherical shape of the earth, the discoverers of perpetual motion, the philosopher's stone, exact laws of meteorology, the exponents of the number of the beast, and other discoveries which the world does not as yet believe in; we find also discussions of the theories and accounts of some of the works of Roger Bacon, Francis Bacon, William Gilbert, Thomas Hobbes, Bishop Wilkins, Sir Isaac Newton, Sir Matthew Hale, Sir Kenelm Digby, Sir George Cornewall Lewis, the early researches of the Royal Society, and many other matters by which the aggregate of our knowledge has been increased. The object of the book is stated to be "to enable those who have been puzzled by one or two discoverers to see how they look in the lump; and incidentally to this we have drawn most clearly a distinction between those who have really made great discoveries and those who have wasted great ingenuity or labour upon what has proved useless; and this is done by showing that it is vain for a man to attempt to improve the knowledge of the world upon any particular subject until he knows all that has been done in that subject. Many of the circle squarers, for instance, are utterly unaware that it has been proved incontrovertibly that it is impossible to arrive at the *exact arithmetical* proportion between the diameter and circumference, but that nevertheless in this very direction the calculation has been carried out to 607 decimal places, a degree of accuracy far greater than is ever required for any practical purposes; so great, indeed, that few persons can realise the extent of its accuracy. It has never, indeed, been proved that it is impossible to produce a *geometrical* equivalent for the circle, but this does not attract so many theorisers. In the collection before us, which is confessedly imperfect, and only consists of the works actually in Professor De Morgan's possession up to 1867, we

find man after man assigning a certain exact sum as this ratio, every man a different amount, and every man confident not only that he alone is right, but that were it not for pride and obstinacy or some such feelings the great mathematicians and astronomers must acknowledge him to be so. These men think they have made lucky hits; but the real discoverers, those whose opinions were at first deemed absurd, but have afterwards convinced the world, such as Galileo, Copernicus, Harvey, and Jenner, have patiently won their way through all previously attained knowledge, making sure of each step as they went along, and then building upon the foundation already laid; thus they have raised themselves above the level of their day. All this is drawn out with much humour and great kindliness of feeling, and so the book is one which is calculated to do great good to those who fancy that they have made great discoveries, whilst they have omitted to acquire the necessary qualifications for discovery, by showing how others have failed in similar pursuits, and also to those who have the power of enlarging our knowledge by encouraging them to proceed in spite of the opposition of the ignorant, after they have assured themselves of all the preliminary steps.

In a work of such varied contents, and so brimful of humour, it is impossible almost to make fair selections. The editor herself evidently has felt this, for whilst she acknowledges that there are repetitions and redundancies, she has found it impossible to cut out these flaws without materially damaging the work. Many of the peculiarities of the writer naturally exhibit themselves in a work of this kind. Many a good story about mathematicians, and especially Cambridge men; many anagrams, evidently a favourite amusement with the author; a few striking remarks about language; and not a few additions to the English language, will afford pleasure to many who would not care much for the mathematical part of the work. A liberal and highly independent view of politics and theology, which one cannot but admire in the man, rather disfigure a work professedly on other subjects. At the same time we miss some discussions which we were entitled to expect, notably the writings of Professor Piazzi Smyth on the Pyramids, who is dismissed with a single casual sentence in the middle of an article on another subject, though his predecessor in the same discussion, Mr. John Taylor, receives longer notice but no criticism of his results. On page 236, immediately before the discussion of the share that Adams and Le Verrier took in the discovery of Neptune, there is a rather glaring misprint: 1826 should be read 1846. On page 385 also there is a discussion of the word aneroid founded on a mistaken derivation; it was formed by the discoverer of the instrument from α, privative, and νηρὸς, moist, because no liquid was employed in this measure of the atmospheric pressure. Our old friend *bogy* is misspelt *boguey*. A few words new to the English language occur occasionally as an "*almamaternal* brother," "*antipharmacopœal*

drenches," a "*sphragidonychangocometical* fellow," "geoplatylogical lectures."

The Orbs Around Us: A Series of Familiar Essays on the Moon and Planets, Meteors and Comets, Suns and Coloured Pairs of Suns. By RICHARD A. PROCTOR, B.A. (Camb.), Hon. Sec. R.A.S., Author of "The Sun," "Other Worlds than Ours," &c. London: Longmans and Co. 1872.

THE well-known author of the interesting essay on "The Sun" has become even still more popular by the publication of subsequent works upon the planetary system. But he has looked back upon his work, and found that the series of descriptions of "Other Worlds than Ours" might, in his estimation, be made to embrace a larger class of readers, if there were appended an introduction or explanation. Careful not to explain too much, Mr. Proctor has supplemented the work just now mentioned with the one before us, on "The Orbs Around us." We will state its salient points. The first essay is intended to elucidate the mysteries of the spectroscope for those who have but a very slight appreciation of the details of this mode of research. The succeeding essays, on the subject of the plurality of worlds, are especially interesting; but even these are exceeded by that entitled "The Rosse Telescope Set to New Work." The value of the work, however, centres in the first essay, because its comprehension includes the capability of progress into more intricate branches of the science of spectral analysis. Mr. Proctor's mission is pre-eminently that of a great teacher of scientific first principles; and his books should be read by all who desire to grasp, if not the detail, at least the liberal ideas of astronomical science. There is no science whose views are so extended, and we may be pardoned if we say that there are few so qualified to impart a knowledge of these views as Mr. Proctor.

The Strength of Materials and Structures. By JOHN ANDERSON, C.E., LL.D., F.R.S.E. London: Longmans and Co. 1872.

THIS treatise is one of the series of the Text-Books of Science now in course of publication by Messrs. Longmans. It is divided into two distinct parts. The first part treats of the natural properties of various materials employed in construction, as far as these qualities and characteristics are of importance to the engineer. In this way the leading peculiarities of cast-iron and wrought-iron, steel, copper, alloys, timber, &c., are described. The student, in the second division of the work, is instructed how to combine materials so as to obtain maximum strength at a minimum of cost and weight.

The work is fully equal to its predecessors, and is characterised

by the usual care for accuracy where tables are concerned. The engineering student should at once add it to his library.

Notes on River Basins. By ROBERT A. WILLIAMS. London: Longmans and Co. 1872.

THIS is a collection of short notes on river basins, drawn up from the works of Petermann and Milner, Mackay, Long and Porter, McLeod, and others. The source, course, drainage, mouth, and tributaries of each river are given; and the area and other details of the lakes of England, Scotland, and Ireland are clearly laid down. The work appears well adapted to the use of pupil-teachers and schoolmasters.

Reports on Observations of Encke's Comet during its Return in 1871. By ASAPH HALL and WM. HARKNESS, Professors of Mathematics, U.S. Navy. Washington. 1872.

THE astronomer and those interested in the science of Astronomy will welcome this able pamphlet. Many difficulties have occurred in the observation, especially in the use of the spectroscope. The spectrum of the comet was very faint; hence it was necessary to remove the photographed micrometer scale of the spectroscope. In its place was inserted a brass plate, pierced with a hole 0·00796 of an inch in diameter, moved by means of a micrometer-screw. The light passing through the hole is reflected from the surface of the prism, and appears, in the field of view of the spectroscope telescope, as a bright disc, with an apparent diameter of 36′ 55″, which can be made to traverse the whole length of spectrum by turning the micrometer-screw. "The illumination of the disc can be adjusted to the brightness of the spectrum under observation with the greatest nicety. If it is required to be very brilliant, the direct light of a lantern may be thrown into the hole: a less degree of brightness may be secured by passing the light through a piece of ground-glass; and finally, the luminosity may be varied down to absolute invisibility by reflecting the light into the hole from the back of the observer's hand held at a suitable angle. This last plan was employed in the case of the comet. The micrometer head is half an inch in diameter, and divided to one-tenth of a revolution, while each complete revolution of the screw moves the brass plate 0·0181 of an inch, which corresponds to an angular distance of 14′ 40·5″."

In using this micrometer, the readings on the line whose place was to be determined were habitually made alternately with readings on a sodium-line, produced by the flame of an alcohol-lamp with a salted wick held before the object-glass of the large

telescope. The measures are thus entirely differential, and there is no risk of errors having been introduced by undetected changes of zero.

We may summarise the results of the observations in a few sentences:—Encke's comet gives a carbon spectrum. There is no polarisation to be detected in the light of the comet. The mass is certainly not less than that of an asteroid. The density of the supposed resisting medium in space, as computed from the retardation of the comet, is such that it would support a column of mercury between $\frac{220}{10^{17}}$ and $\frac{285}{10^{20}}$ of an inch in height. There is some probability that the electric currents which give rise to auroras are propagated in a medium which pervades all space, and that the spectrum of the aurora is, in reality, the spectrum of that medium. It is not improbable that the tails of all *large* comets will be found to give spectra similar to that of the aurora, although additional lines may be present.

In conclusion it may be said that, from the clearness of the detail, this pamphlet will be useful to the astronomical student.

The Forces of Nature. A Popular Introduction to the Study of Physical Phenomena. By AMEDEE GUILLEMIN. Translated from the French by Mrs. NORMAN LOCKYER; and Edited, with Additions and Notes, by J. NORMAN LOCKYER, F.R.S. London: Macmillan. 1872.

THE progress of Physical Science is nowhere more clearly apparent than in a comparison of the mode of producing its records. The soberly bound volumes of half a century ago are not more likely to be banished to the higher shelves of our book-cases because the theories they expound are obsolete, than they are to be superseded by the luxuriously printed and illustrated books in which the philosopher of to-day declares the laws of Nature according to his present lights. It is fit it should be so. Delicate instruments and logical reasoning should have their details drawn with a loving hand. Much of the science of yesterday lived grimly and darkly in its own study; the science of to-day throws its light upon all, and as a natural truth should be shown as it appears, in its own attractive form. For instance, why should not the diary of a journey through the realms of light—"a fairy-like, enchanted world, a world of wonders, where rubies, sapphires, topazes, and all kinds of precious stones send forth their fires, where every object is of incomparable beauty and splendour"—receive the most efficient ornament the aid of art can impart. Such a luxury, if it is luxury, is a practical one, for it raises in the mind of the student the enthusiasm which is necessary to render him a lover of not only Nature, but, as well, of Nature's laws.

The book which we have to notice is of French origin, from the pen of the celebrated Guillemin; its appearance in England is due to the united labours of Mrs. and Mr. Norman Lockyer. We need but say that it contains all the information of other works on Physical Science, under the heads of Gravity, Sound, Heat, Electricity, and Light, and that this information is further aided by the most eloquent and vivid illustrations we should think the power of the artist could attain."

A Treatise on the Building and Ornamental Stones of Great Britain and Foreign Countries. By EDWARD HULL, M.A., F.R.S., Director of the Geological Survey of Ireland, Professor of Geology in the Royal College of Science, Dublin. London: Macmillan and Co. 1872.

THOSE interested in the geological distribution and mineral character of the building and ornamental stones employed in the erection of ancient and modern structures will be pleased to find that the materials, which have hitherto been scattered so widely, have been brought within the limits of a single volume. Building and ornamental stones have not, we believe, been described in a complete manner, nor with any particular scientific arrangement. The engineer or student in Ireland is better provided for by Mr. G. Wilkinson's "Ancient Architecture and Practical Geology of Ireland; in France, M. T. Chateau has published his "Technologie du Bâtiment."

Under the general divisions of granitic, porphyritic, greenstone, and serpentinous rocks, marbles, alabasters, the rarer ornamental stones, calcareous and siliceous stones, tufaceous stone and slates, Mr. Hull deals with the varieties found in different parts of the world, in a manner clear, concise, but sufficiently detailed. One of the concluding chapters, on the selection of building stones with special regard to climate and the nature of the atmosphere, is well worthy the attention of the practical engineer. In each and all its departments the work is a valuable addition to our engineering literature.

Life of Richard Trevithick, with an Account of his Inventions. By FRANCIS TREVITHICK, C.E. Vol. II. London: E. and F. N. Spon. 1872.

THE fertility of Trevithick's inventive powers appears to even greater advantage in this second volume than in the first, which we recently had occasion to notice. Although so many of Trevithick's ideas have been superseded by later inventions, there are several schemes which in the present day would afford valuable application. The engineering student should read the work as a

record of many difficulties surmounted, and as many more avoided; it embodies both precept and example.

A Manual of Palæontology. By HENRY ALLEYNE NICHOLSON, M.D., D.Sc., &c., Professor of Natural History and Botany in University College, Toronto. Edinburgh and London: Blackwood and Sons. 1872.

DR. NICHOLSON'S object has been to furnish the student of geology and the general reader with a compendious account of the leading principles and facts of the vast and ever-increasing science of Palæontology. The work is divided under four heads:—the first includes a general account of the principles upon which the palæontological observer proceeds; the second treats of the past history of the animal kingdom, devoting much more space than is generally accorded to the consideration of invertebrate groups; under the third head is given a comprehensive view of palæobotany, or the past history of the vegetable kingdom; while, finally, the author applies the principles of palæontological science to the elucidation of the succession of the stratified deposits of the earth's crust. To say that this is the best handbook yet produced by the prolific pen of Dr. Nicholson is to accord the highest praise. The work is profusely and well illustrated.

Elements of Zoology. By ANDREW WILSON, Lecturer on Zoology, Edinburgh. Edinburgh: Adam and Black. 1873.

THIS is a manual intended to convey the principle of the division of zoological science to the student of an elementary course. The explanation is terse, but sufficient; the illustrations are numerous and well selected.

A Manual of Elementary Chemistry, Theoretical and Practical. By GEORGE FOWNES, F.R.S., late Professor of Practical Chemistry in University College, London. Eleventh Edition. Revised and Corrected by HENRY WATTS, B.A., F.R.S. London: J. and A. Churchill. 1872.

THE eleventh edition of this well-known manual of chemistry presents some marked alterations. The work, under the careful editorship of Mr. Watts, fully keeps pace with the progress of chemical science. But the volume appears overgrown: if the matter were divided under the heads of organic and inorganic chemistry, and each portion included in a separate volume, the manual would take a much handier form. The present volume

is too bulky to be held with ease. The use of a bolder type is a very considerable improvement. It is unnecessary to recommend the work more particularly.

Elementary Geology. A Course of Nine Lectures. By J. CLIFTON WARD, F.G.S., Associate of the Royal School of Mines; of Her Majesty's Geological Survey. London: Trübner and Co. 1872.

MR. WARD is already well known to the scholastic world by his work on Elementary Natural Philosophy. The present work is founded upon a similar plan, and is specially adapted for its proposed use by junior students and in schools.

Notes for My Students. Magnetism. By WILLIAM J. WILSON, F.C.S. London: J. Bale and Sons. 1872.

THIS little work is admirably adapted for the use of either the advanced or elementary student. It is very clearly and concisely written, and comprises much useful information.

The Causation of Sleep. By JAMES CAPPIE, M.D. Edinburgh: Thin. 1872.

DR. CAPPIE, in this essay, gives many novel and ingenious suggestions upon an interesting subject. It would be tedious to detail the many original views differing in some degree from the accepted opinions on a physiological subject. We recommend our readers to examine for themselves these arguments, which are clearly and logically stated in a sufficiently agreeable form.

PROGRESS IN SCIENCE.

MINING.

To-day—January, 1st, 1873—the two Mines Regulations Acts of last session come for the first time into operation. In conformity with certain sections of these Acts, every owner, agent, or manager of a mine must, before a specified date, forward to the inspector of his district a return of the annual produce of his mine. A complete change is, therefore, about to be introduced in the system of collecting our mineral statistics. Hitherto these valuable returns have been purely voluntary contributions, obtained through the personal influence of Mr. Robert Hunt, F.R.S., Keeper of Mining Records. As far back as 1847, statistics of this kind were for the first time collected and published by Mr. Hunt, and since 1853 the volumes have been regularly issued year by year—gradually growing in fullness and accuracy until they have assumed their present comprehensive form. In view of the compulsory system introduced by the new Acts, we may regard the volume for 1871*—which has appeared during the past quarter—as representing the last of the returns contributed by the courtesy of our British mine owners. From this volume we extract the following summary, showing the number of mines working in 1871, and the amount and value of the ores which they produced:—

Number of Mines.	Mineral.	Tons.	Cwts.	£
2760	Coal	117,352,028	—	35,205,608
210	Iron ore†	16,334,888	14	7,670,572
122	Copper ore	97,129	—	387,118
145	Tin ore	16,272	—	1,030,834
241	Lead ore	93,965	17	1,155,770
47	Zinc ore	17,736	10	56,330
33	Iron pyrites	61,973	—	64,987
1	Silver ore‡	5	—	421
16	Arsenic	4,147	15	15,519
9	Gossans, ochres, &c.	697	5	1,396
1	Wolfram and tungstate of soda§	20	—	228
1	Nickel‖	2	—	98
1	Bismuth¶	—	2	14
2	Fluor-spar	51	10	26
4	Manganese	5,548	1	22,958
1	Cobalt-ore§	3	—	120
	Barytes	5,512	8	3,539
	Clays, fine and fire	1,255,000	—	475,000
	Earthy minerals	—	—	600,000
	Salt	1,505,725	—	752,862
	Coprolites	36,500	—	51

Total value of the minerals produced in the United Kingdom in 1871 .. £47,494,400

* Mineral Statistics of the United Kingdom of Great Britain and Ireland for the year 1871. With an Appendix. By Robert Hunt, F.R.S. London: Longmans and Stanford. 1872.

† "It has not been possible in every case to determine whether the return has been for *calcined* or *uncalcined* ore. The actual production of *raw* ore will probably be in excess of this quantity. Estimating the quantity of pig-iron at 2¾ tons of ore for each ton of iron, and deducting foreign ore, 'burnt ore,' and 'cinder' used, the quantity will be about or slightly above 17,000,000 tons."

‡ From the Queen Mine, Calstock, Cornwall.

§ From East Pool Mine, near Redruth, Cornwall.

‖ From Silver Mine, Bathgate, Linlithgowshire.

¶ From Dolcoath Mine, near Redruth, Cornwall.

From the official reports of the Inspectors of Coal Mines for 1871, we learn that 826 fatal accidents occurred in connection with our collieries during the year. It is true that this number is slightly less than the corresponding figures for the previous year; but whilst the 830 accidents of 1870 resulted in the loss of 991 lives, the 826 accidents of 1871 represent unhappily not fewer than 1075 lives. It appears that of every 345 colliers employed in 1871, one man perished by these accidents; or, to put the figures in another light, it may be said that one miner's life was sacrificed for every 109,246 tons of coal raised in that year. On analysing the list of fatal accidents, we find that 52 of them occurred through explosions of fire-damp, and resulted in 269 deaths; whilst 426 may be referred to falls of the coal, ironstone, or roof—a class of accidents which caused the loss of 435 lives. The remaining deaths were due to casualties in the shafts, and to miscellaneous accidents, both underground and at the surface. Let us hope that the working of the new Act may diminish each year this grim catalogue of colliery accidents.

Many of the Government inspectors introduce into their reports highly valuable suggestions, which merit the studious attention of all who are practically interested in our mining industries, and especially those who have the lives of our coal-miners in their charge. It is pleasing to mark the spirit in which some of the inspectors refer to the benefits which must accrue to mining officers from a scientific education, and to the influence which such training must needs exert on the intelligent discharge of their responsible duties. The means of acquiring such training are, however, not yet sufficiently extensive. Thus, Mr. Lionel Brough, after alluding with satisfaction to the establishment of the College of Physical Science at Newcastle-on-Tyne, maintains that "every centre should by right possess one of those most valuable educational establishments. The underground operations of Great Britain exceed those of any other nation in the world; therefore educational means should be provided proportionate to its immense mining industry."

As colliery explosions are often the indirect result of diminished atmospheric pressure, Mr. J. A. R. Newlands has suggested, with the view of preventing such calamities, that the air in coal mines should be maintained at a constant pressure by artificial means. To this end he proposes to cover the mouths of both the upcast and downcast shafts by air-tight chambers, sufficiently large to allow all the surface-work at the pit's mouth to be carried on within their walls. These chambers should be put in connection with powerful air-pumps, worked by steam-power, and a current of fresh air thus forced through the workings. This current could be so regulated that any desired degree of ventilation might be attained, while the air, if necessary, might be cooled, before passing into the pits, by compression in cylinders surrounded with cold water. When fire-damp makes it appearance, air should be drawn out of the mine, and the pressure in the workings thus diminished, so as to release, in the absence of the miners, any imprisoned gas. It is believed that in many collieries, dangerous accumulations of fire-damp might be prevented by the simpler plan of partially exhausting the air periodically, and then forcing a current of fresh air into the pit, so as to sweep through the entire system of workings. Instead, therefore, of erecting air-tight chambers, it would in such cases be merely necessary to cover the mouth of each shaft with an iron plate, having an aperture by which it could be put into communication with the pump for either exhausting or forcing-in the air.

We had occasion last quarter to mention that the Committee appointed by the War Office to report upon lithofracteur had come to the conclusion that this explosive is not perfectly safe under certain conditions. It is only fair, therefore, that we should now call attention to the fact that a different view has been taken by the Belgian Government, and that a concession has recently been granted for the transport and storage of lithofracteur in Belgium. A series of important experiments has been satisfactorily performed, on a large scale, with this substance before some of the chief mining and engineering authorities in that State. These experiments were made in some quarries of greenstone at Quenast, about 18 miles from Brussels, where a hard compact

rock is largely quarried for paving and road-making. Without entering into the details of these experiments, which were conducted by Prof. Engels, the inventor of lithofracteur, we may say that they satisfactorily showed the extraordinary power and value of this explosive, whether for mining or for military blasting, and also demonstrated its incapacity to explode by fire or by ordinary percussion.

It seems highly probable that Eastern Australia will soon enter into competition with Cornwall and "The Straits" as a great tin-producing country. A report on the recent discoveries of tin-ore in the colony of Queensland was presented by Mr. F. T. Gregory at the opening meeting of the Geological Society this session. According to this document, the ore has already been found distributed over an area of about 550 square miles of granite country in the neighbourhood of the head-waters of the Severn River and its tributaries. Many small tin-lodes have been traced, invariably in association with a red granite; but the richest sources of tin are the deposits in the beds of streams and in the alluvial flats on their banks.

In the adjacent colony of New South Wales, and immediately adjoining the stanniferous region of Queensland, important discoveries of tin-ore have also been recently made, and are in course of rapid development. Some interesting observations on these discoveries have been transmitted to this country by Mr. G. H. F. Ulrich. The tin-yielding region of New South Wales forms an elevated plateau in the district of New England, and consists mainly of granitic and basaltic rocks, associated with metamorphic slates and sandstones. At the workings of the Elsmore Company, north-west of the Macintyre River, the granite is traversed by veins of quartz containing tin-stone, and by dykes of a softer granite, so rich in ore as to yield masses of oxide of tin up to at least 50 lbs. in weight. Capping the granite range is a layer of recent tin-bearing detritus, from 6 to 24 inches in thickness, and yielding from 3 ozs. to more than 2 lbs. of tin-ore per dish of about 20 lbs. Beneath this there occurs an older drift, which in some parts has yielded as much as 6 lbs. of ore per dish, whilst other parts are comparatively poor. Though the full development of the new mining industry thus established in this part of Australia may be to some extent restricted by lack of a sufficient supply of water, yet Mr. Ulrich considers it not unlikely that the production of tin-ore from this region will eventually reach, or even surpass, that of all the old tin-mining countries of the world. Mr. Daintree, who is well acquainted with the colony of Queensland, calculates that the value of the deposits of stream-tin in that colony must be about £13,000,000 sterling! And, assuming that the neighbouring colony of New South Wales possesses deposits of equal value, he estimates that the stream-tin of this eastern part of Australia amounts to about twenty-five times the annual production of Cornwall. The discovery of tin in New South Wales is said to be due to the Rev. W. B. Clarke, who in 1849 predicted the occurrence of this metal from the character of some of the local granites, and in 1853 reported the actual discovery of tin-ore in the neighbourhood of the Severn River. It is only lately, however, that these discoveries have excited any attention.

The first half-yearly part of a new official periodical—the "Annals of Mining in the Dutch East Indies"*—has lately been published. It contains some valuable geological, mining, and metallurgical articles, including an excellent paper on an important tin district in the island of Banca.

METALLURGY.

Perhaps the best idea of the importance of metallurgy among the industries of this country may be obtained by consulting the annual volumes of statistics issued from the Mining Record Office by Mr. R. Hunt, F.R.S. The returns for 1871 have been published during the past quarter, and from thence we learn that the value of metals produced from ores raised in this country during that

* Jaarboek van het Mijnwezen in Nederlausch, Oost-Indië. Uitgegeven op last van zijne Excellentie die Minister van Koloniën. Eerste Jaargang; eerste Deel. 1872.

year amounted to upwards of £20,000,000 sterling. The following summary exhibits the quantities and values of the several metals smelted from British ores in 1871:—

Pig-iron	Tons	6,627,179	£16,667,947
Copper	"	6,280	475,143
Tin	"	10,900	1,498,750
Lead	"	69,056	1,251,815
Silver..	Ozs.	761,490	190,372
Zinc	Tons	4,966	92,743
Other metals (estimated)			3,000
			£20,179,770

Certain improvements in the metallurgy of manganese have recently been effected by Mr. Hugo Tamm, and fully described in the "Chemical News." The ore employed in these investigations was an impure binoxide of manganese, containing 79·5 per cent of peroxide of manganese, 6·5 of peroxide of iron, 3·5 of water, and 10·5 of gangue, with traces of phosphate of lime. To obtain metallic manganese, 1000 parts of this ore are mixed with 91 of lamp-black or soot, and with 635 parts of a mixture described as "green flux." This is prepared in the following way:—A mixture is made of 63 parts of ground glass, 18½ of quick-lime, and 18½ of fluor-spar. Of this mixture 34 parts are taken and incorporated with 5½ of lamp-black and 60½ of native peroxide of manganese. On smelting this mixture, metallic manganese is obtained, accompanied by an olive-green slag: this slag, when ground, forms the green flux previously described. The charge of ore, flux, and carbonaceous matter, in the proportions indicated above, and moistened with oil, is introduced into a refractory crucible lined by a mixture made of 3 parts of plumbago and one of loam or fire-clay worked into a thick paste with water. The crucible is heated in a wind or blast-furnace, and a button of manganese obtained, together with the slag previously described. The metal reduced by this method is not pure manganese, but a product which the author designates as "cast manganese." A specimen of this contained—Metallic manganese, 96·9; iron, 1·05; aluminium, 0·1; calcium, 0·05; phosphorus, 0·05; sulphur, 0·05; silicon, 0·85; carbon, 0·95. The cast manganese may be refined by Berthier's process, which consists in re-melting the coarsely-powdered crude metal with carbonate of manganese. A sample of the refined metal obtained by this treatment had the following composition:—Manganese, 99·91; iron, 0·05; silicon, 0·015; carbon, 0·025. Mr. Tamm suggests that the cast manganese might be economically employed in certain operations as a good substitute for the alkaline metals.

A patent has been granted to Messrs. T. W. Gerhard and J. Light, jun., for the production of iron and steel from a certain preparation which they call "iron-coke." This is a mixture of powdered ore, or iron-scales, with a bituminous substance, such as pitch, and with carbonate of lime. Cast-iron may be obtained by smelting the iron-coke with ground coal or other carbonaceous matter. Wrought-iron may be procured by the reducing action of carbonic oxide generated in a combustion chamber connected with the furnace.

An improved method of lining rotatory puddling furnaces has been patented by Mr. Danks. Lime and oxide of iron, or silicate of iron, mixed in certain cases with soda, potash, or even common salt, are worked-up into the consistence of a stiff mortar, with which the revolving cylinder is lined. When this coating has become dry, iron ore is introduced into the furnace and melted, thus forming a vitreous lining. More ore, or oxide of iron, is then melted, and lumps of ore thrown into the molten mass, so that, when the liquid sets, the ends of these lumps project from the surface. In this condition the furnace is ready for puddling.

Certain salts—such as the alkaline nitrates and chlorates—are applied by Mr. R. Elsdon to the conversion of cast-iron into wrought-iron or steel by

causing them to act on the *upper* surface of the molten pig-iron, which is placed in a peculiar syphon-shaped converter.

Mr. W. Dingley has lately patented the use of sulphate of soda, in the crude state of *salt-cake*, for the purification of iron. A small quantity of the salt is thrown on to the surface of the molten iron during the operation of puddling —a dose of about 12 ozs. being recommended for each heat of 4 or 4½ cwts. of metal.

Some improvements in the separation of silver and gold from lead have been announced by Messrs. Risway and Pauville, of Paris. The argentiferous or auriferous lead is treated with magnesium or aluminium, either alone or alloyed with zinc, and the rich scum thus obtained is amalgamated with mercury. The inventors state that they are able to regenerate the metals which have been used in the process of extraction, and have thus greatly reduced the expense of separating the precious metals from the lead.

Mr. F. Claudet has presented to the Academy of Sciences of Paris a memoir on his process of extracting gold and silver from burnt coppery pyrites—a process extensively conducted at Widnes by Mr. J. A. Phillips. The treatment consists essentially in roasting the burnt ore at a low temperature with common salt, lixiviating the product with water acidulated with hydrochloric acid, precipitating the silver by iodide of potassium, and decomposing the iodide of silver by metallic zinc. It is unnecessary, however, to enter into details of the process, as it was described by Mr. Phillips at the Liverpool meeting of the British Association. We learn from Mr. Claudet that in 1871 not less than 16,300 tons of burnt pyrites were thus treated at Widnes, and yielded 333·242 kilogrammes of silver and 3·172 kilogrammes of gold.

A capital account of tin-smelting, as practised in Banca, appeared in the first part of the new Dutch periodical on East Indian Mining. The description is written by Van Diest, and is illustrated by an effective chromo-lithograph representing the Chinese method of tin-smelting as practised at night.

MINERALOGY.

Last quarter we had occasion to refer briefly to the discovery of a new lead-bearing mineral called *Maxite*. A full description of this interesting species has since been published by Dr. Laspeyres.* Herr Max Braun, of the Vieille Montagne Zinc Mines, near Aix-la-Chapelle, having visited the lead mine known as the Mala Calzetta, near the town of Iglesias, in Sardinia, brought home with him certain specimens of the new mineral, which were at first taken for mendipite or chloro-carbonate of lead. It was soon found, however, that no chlorine was present, and a full analysis has since revealed the following composition:—Water, 1·866; carbonic acid, 8·082; sulphuric acid, 8·140; protoxide of lead, 81·912. From these figures the following formula may be deduced (using the old atomic weights):—

$$5(PbO.SO_3)+9(PbO.CO_2)+PbO.5HO.$$

This formula corresponds to 31 per cent of sulphate of lead, 49 of carbonate of lead, and 20 of hydrated oxide of lead. Up to the present time we believe that the mineral has not been found *in situ*, but the few specimens yet known have all been obtained from the dressing-floor at the mine. During the process of dressing, the crystals have of course been subjected to attrition, and hence the surfaces are much rubbed and rounded, so that no crystalline faces have yet been found sufficiently distinct to admit of measurement. It is inferred, however, from its cleavage and from its optical properties, that maxite belongs to the rhombic system. It presents the form of a colourless or greyish-yellow crystalline substance, with a pearly adamantine lustre on the cleavage planes. Its hardness is almost equal to that of calc-spar, and its specific gravity is 6·874. Optical examination shows that it is a negative

* Leonhardt und Geinitz's Neues Jahrbuch für Mineralogie, U.S.W., 1872, Heft 5, p. 508; Journal für praktische Chemie, 1872, Heft 10, p. 470.

doubly-refracting mineral. In many of its properties it is closely allied to the rare British mineral, leadhillite, from which it differs, however, in density, in the presence of water, and in certain other characteristics.

A new ore of mercury from Guadalcazar, in Mexico, has been described by Dr. T. Petersen, under the name of *Guadalcazarite*. It is a compact or crypto-crystalline iron-black mineral, with a metallic lustre and a black streak. The specific gravity is 7·15. An analysis yielded—Sulphur, 14·58; selenium, 1·08; mercury, 79·73; zinc, 4·23; with traces of cadmium and iron. Guadalcazarite is, therefore, a double sulphide of mercury and zinc, corresponding to the formula $6HgS+ZnS$, but with part of the sulphur replaced by selenium, and perhaps a small proportion of the zinc by cadmium.

In a recent number of the "Annales des Mines," M. Bertrand describes a curious yellow or reddish substance from the province of Los Bordos, in Chile, and found to contain the chlorides of silver and mercury, with oxide of mercury. It is believed that this substance is a mixture of two mineral species. One of these is a double chloride of silver and mercury, containing $AgCl$ 40·69, and Hg_2Cl 59·31 per cent: this new species is called *Bordosite*. The other constituent is ordinary protoxide of mercury, which in this native form is to be termed *Hydrargyrite*.

Under the name of *Syngenite*, Zepharovich has lately described a new mineral from Kalusz in Galicia. It occurs in the form of colourless tabular crystals, much resembling those of gypsum, and associated with crystals of sylvine, or chloride of potassium, in the salt mines of Kalusz. Syngenite is a hydrous double sulphate of calcium and potassium, somewhat resembling polyhalite, from which it differs in that it contains scarcely any sulphate of magnesium. The native crystals are almost identical with those of the similar product formed in the laboratory; but though belonging to the rhombic system, they affect a curiously deceptive monoclinic habit.

Herr H. Grüneberg has communicated to the German Chemical Society a memoir on the properties and economic applications of the mineral *Kieserite*. This is a hydrous sulphate of magnesium, containing only a single molecule of water, and hence differing from ordinary crystallised Epsom salts. Kieserite occurs abundantly among the "abraum salts," or deposits of magnesium and potassium salts in the upper beds of the salt mines at Stassfurt, near Magdeburg. It is extensively used in Manchester for dressing cotton and other fabrics, and it is also valued as a manure. Kieserite and common salt, reacting at a low temperature, furnish sulphate of sodium—hence another application of the mineral. Grüneberg has succeeded in preparing a double sulphate of magnesium and calcium by igniting a mixture of kieserite and gypsum, and has introduced this double salt as a hard and durable artificial stone.

At Nohl, near Kongelf in Sweden, Professor Nordenskjöld has discovered a new mineral which he terms *Nohlite*. This is a hydrated niobate of yttrium, uranium, zirconium, calcium, iron, &c. The mineral resembles the Uralian Samarskite, but contains more than 4·5 per cent of water.

Some few years ago, Mr. Ulrich, in his excellent notes on the mineralogy of Victoria, described some beautiful crystals of *Herschelite* from Chambers's basalt quarries at Richmond, near Melbourne. Samples of these crystals have lately been analysed by Herr Kerl in the laboratory of the University of Göttingen with the following results:—Silica, 43·7; alumina, 21·8; lime, 8·5; soda, 3·5; potash, traces; water, 22·2. Compared with the typical herschelite of Sicily, the Australian mineral contains much less silica, a larger proportion of water, and a notable difference in the proportion of lime and alkalies. Indeed a specimen of Sicilian herschelite contained only 0·31 per cent of lime, but as much as 8·84 of soda and 4·28 of potash. These differences in chemical composition have been considered sufficient to justify the separation of the Australian mineral from the species herschelite, in spite of the close agreement in the crystallographic characters of the two substances. Herr

M. Bauer, of Göttingen, has, therefore, proposed to distinguish the Australian mineral as *Seebachite*—a name complimentary to Professor Karl von Seebach. At the same time, it must be confessed that the composition of the two minerals may perhaps be eventually reduced to a general formula, in which event a relation might be traced between herschelite and seebachite similar to that which obtains between natrolite and mesolite.

A curious instance of the occurrence of hemimorphism in a crystal of calc-spar—perhaps an unique example—has also been described by Bauer in the *Zeitschrift* of the German Geological Society. The specimen in question occurs in a group of crystals of calcite from Andreasberg in the Hartz—a locality well known to the mineralogists for the beauty of its crystallised calcite. Most of the crystals in this group are attached by one end to the matrix, and hence it is impossible to compare the characters of the two extremities; but it happens that one crystal has curiously grown across another in such wise that the two ends of the former crystal are free, and hence admit of observation. The hemimorphism consists in one end being terminated simply by the flat basal plane, whilst the other extremity exhibits a complicated set of rhombohedra and scalenohedra. As the occurrence of hemimorphism is usually correlated with pyro-electric properties, the crystal of calcite was heated to 150° C., but without any development of electricity. Exposure to a higher temperature was forbidden by fear of damaging so interesting a specimen.

Mr. C. Horner has communicated to the "Chemical News" a short note, announcing the discovery of the rare metal didymium in a specimen of pyromorphite, or phosphate of lead, from Cumberland.

Dr. Gladstone, F.R.S., has succeeded in preparing microscopic specimens of filiform silver, strongly resembling the characteristic threads of the native metal well known to mineralogists as occurring in calcite at Kongsberg in Norway and in Chili. The artificial specimens were reduced from a solution of nitrate of silver by suboxide of copper, and it is suggested that the native silver may have been reduced by a similar reaction in nature.

ENGINEERING—CIVIL AND MECHANICAL.

Guns and Armour.—The results of the *Glatton-Hotspur* experiments recorded in our chronicles of last quarter have recently come under discussion at Portsmouth by the Naval Professional Association, on the 1st November last, when a paper on the subject was read by Commander W. Dawson, R.N. From this paper it appears that the defeat of the gun by the turret was in strict conformity with well-known mechanical principles, with previous Shoeburyness experiments, and with Woolwich calculations. The shots fired at the *Glatton* were from a 12-inch 25-ton gun, rifled on the French or Woolwich system, and the results of the firing fully confirmed Captain Hood's remark as to the well-known "inaccuracy of flight now observed in a 12-inch gun of 25 tons at very short ranges." Now, from a diagram published by Mr. N. Barnaby, the Chief Naval Architect, it appears that the force required to perforate the front of the *Glatton's* turret, at right angles, with a 12-inch projectile, is 7378 foot-tons. This would be exerted at 200 yards (the distance in question) by a 600 lb. shot, which left the gun at the rate of 1357 feet per second; or by a 700 lb. shot projected with an initial velocity of 1252 feet. But the 600 lb. shot actually employed, having those short stud rifle-bearings which, it is officially stated, have "decidedly the lowest velocities," left the gun with only 1300 feet velocity; and, travelling 23 feet per second slower at 200 yards distance, it struck the turret a blow of only 6788 foot-tons. In order that this projectile should perforate the front of the *Glatton's* turret, it must leave the gun with 1357 feet velocity, or with 57 feet greater initial velocity. The same object might be attained by propelling a 700 lb. projectile from the same gun with 105 feet less velocity than that requisite with a 600 lb. shot. Without following Commander Dawson through his proofs and arguments, in which he clearly traces the defeat of the gun to the defective

system of rifling employed, and the short stud bearings of the shot necessitated by that system, it is sufficient to state that his conclusions are fully borne out by other authorities. Mr. Charles Merriman, F.R.S., the principal of the Royal School of Naval Architecture at South Kensington, says that "the consent of all mechanicians and engineers with "whom he has ever conversed was absolutely unanimous in the condemnation of the Woolwich system of rifling, and that he had never heard of any serious defence of it." The results of the *Glatton* experiment also confirm the unanimous testimony of unbiassed artillerists, of mechanicians, and of mathematicians, that the rifle system which "has decidedly the lowest velocities" has necessarily the least penetrating power; and Admiral A. C. Key, C.B., F.R.S., the former Director-General of Naval Ordnance, lays it down as a rule that without "penetrating power, at ranges up to 1200 yards, all other qualities are useless."

Railways.--Among the new lines of railway now approaching completion, we may notice the Mexican Railway, connecting the port of Vera Cruz with the capital of the country. It is 263 miles in length, with a branch 29 miles long to the city of Puebla. Leaving the port of Vera Cruz, the line runs up towards the mountains of the Chiquihuite, rising about 1600 feet in a distance of 53 miles; it then reaches the Tierra Templada, at a height of 4000 feet in 84 miles, and finally overcomes an elevation of 8043 feet on the borders of the great Mexican plateau, or Tierra Fria. In reaching this last elevation some very heavy work presents itself; steep gradients of 1 in 25 combined with curves of 350 feet radius, are frequent over a distance of 22 miles, and there are several viaducts and bridges of considerable size. The gauge of the line is 4 feet 8½ inches, and the engines, carriages, &c., will be all adapted to the sharp curves they will have to traverse.

The official list of new projects to be submitted to Parliament during the ensuing session comprises 280 plans of all classes, of which number 159 are railway schemes, 13 are tramway bills, and 65 are bills of the miscellaneous class. The railway schemes are chiefly provincial, although there are some which affect the metropolis. With regard to the latter there are, first, the City and West End Railway, which is a proposed line from the Metropolitan Railway at the Kensington Joint Station to Farringdon Street, the route being by way of Great Windmill Street to the Metropolitan Railway at the Farringdon Road Station. There is a new street between Tichborne Street and Rupert Street, and another between Holborn and Great Queen Street in connection with this scheme, besides which it is proposed to widen several streets along the route. The East and West Metropolitan Junction and Cannon Street Railway is a scheme for a line from the Metropolitan District Railway at Cannon Street to the Metropolitan Railway at Aldgate, to the East London Railway, and to the North London Railway at Bow. The Metropolitan and St. John's Wood Railway Company are seeking to connect their line with the Hampstead Junction Railway and the Midland Railway, and to construct a branch line to Kingsbury. The Hammersmith Extension Railway is a proposed line from the Metropolitan District Railway at Kensington to the Broadway, Hammersmith. The London Central Railway Company are seeking powers to form junction lines with the Great Northern Railway near the passenger station at King's Cross, and to effect a junction with the Metropolitan Railway near Osnaburg Street, Euston Road, and another junction with the same railway near Upper Fitzroy Street.

The Brighton, Eastbourne, and London Railway, which has already been before Parliament, is again brought forward. The Great Eastern and Felixstow is a line from the Westerfield station of the Great Eastern Railway to Felixstow. The Great Northern Railway propose to construct branches from their own line at Fletton to the London and North Western at Orton, and between their Nottingham and Grantham Branch at Barrowby to their main line at Barkston; they also seek powers for a railway from the termination of their authorised line at Melton Mowbray to Leicester, with three branch lines.

The Great Western Railway Company are seeking powers to make a line from Stourbridge to Kidderminster and Bewdley, some lines at Wrexham,

sidings at Paddington and Bristol, and the extension of the Llwynennion branch. By another bill they propose to construct branches from the Cornwall Railway to Devonport, and from the West Cornwall Railway to St. Ives. The London and North Western Company are applying for powers to add considerably to their system by the construction of lines in Middlesex, Northampton, Rutland, Huntingdon, Stafford, Chester, York, Monmouth, Carmarthen, Glamorgan, and Carnarvon, which are too numerous to mention in detail. The South Western Railway propose a considerable extension of their system by the construction of lines in Bucks, Surrey, Berks, and Southampton. The Midland Railway propose several short branches at Stockingford, Kingsbury, Ripley, Teversall, Duckmanton, Skegley, Bestwood-Park, Holbeck, and one in connexion with the Metropolitan Railway at Whitecross Street. The South Eastern Railway Company are applying for powers to construct new lines at Rochester and Chatham, a junction line between the South Eastern, New Tunbridge, and Paddock Wood and Maidstone lines. The proposed Staines and West Drayton Railway is a line leaving the great Western line at Hillingdon, and terminating by a junction with the Windsor branch of the South Western Railway at Staines. The Swindon, Marlborough, and Andover Railway is a line from the Great Western, at Swindon, to the London and South Western at Andover.

Bridges.—An important engineering work is now under construction across the Thames, at Chelsea, known as the Albert Bridge. The principle of its construction is that known as Mr. Ordish's rigid suspension principle; this system consists in suspending the main girders, which carry the roadway, by straight inclined chains, which are maintained in their proper position by being suspended by vertical rods, at intervals of 20 feet, from a steel wire cable. The bridge, when completed, will have a total length of 710 feet, and a width of 41 feet between the parapets. These will be formed of the main girders, which are of wrought-iron, 8 feet deep and continuous, the upper portions being ornamentally perforated in order to lighten and improve the appearance of the structure. The main girders will be connected transversely by cross girders placed 8 feet apart, and on these will be laid the planking for the carriage roadway. There will be four towers carrying the main chains of the bridge, and they will be placed in pairs, each pair being connected at a height of 60 feet from the platform level by an ornamental iron arch. The towers are of cast-iron, and consist each of an inner column 4 feet in external diameter, surrounded by eight 12-inch octagonal columns placed 12 inches from the central shaft, the whole group being connected together at intervals by disc pieces or collars of cast-iron. The bridge is divided into a centre and two side openings, the former a span of 400 feet, and the latter 155 feet each. The foundations of the piers consist of cast-iron cylinders, the bottom or cutting ring being 21 feet in diameter, and they are the largest cylindrical castings ever made in one piece. From these the cylinders gradually taper to 15 feet in diameter at the level at which the towers commence. The cylinders are sunk down into the London clay, and then filled in with concrete.

A paper has recently been read before the American Society of Civil Engineers relative to the problem of how to sustain and maintain in position the arch ribs of the Illinois and St. Louis bridge during the progress of its construction. The most prominent novelty in the plan adopted consists in the absence of all scaffolding or trestling standing in the river, excepting only for a very short distance immediately adjoining the piers and abutments, substituting therefor a suspending system from above; also in using the inherent stiffness of the arch ribs themselves as caulilevers, aided, if found necessary, by temporary "guys" from the piers and abutments, to support the derricks and stages from which to project forward the successive sections of the ribs. Space will not admit of our entering further upon this subject at present, but we shall return to it again upon a future occasion, as it is one of extreme novelty and well deserving of further notice.

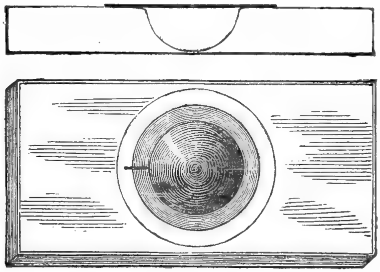
Channel Steamer.—Whilst the various schemes for rendering communication between this country and the Continent are under consideration,—whether

by means of a tunnel or otherwise,—Mr. Bessemer has recently introduced a new type of steam-vessel, the leading principle of which is to neutralise the wave-action of the sea to such an extent as that a portion of the ship—namely, the state cabin—shall remain always at rest, thus overcoming all the present incentives to sea-sickness. The pitching action of the vessel is proposed to be overcome by giving it a very low freeboard at either end, and driving it through the waves instead of allowing it to mount them. The rolling motion is overcome by suspending the saloon at each end, and at two intermediate points, upon steel axes, supported upon standards. To prevent the saloon from being affected by the oscillation of the vessel, or its equilibrium from being disturbed by the movements of the passengers, it is fitted with hydraulic gear, by means of which its position with respect to the vessel is placed under perfect control, an attendant, having a spirit-level before him, being enabled, by the manipulation of a single lever, at all times to keep the floor of the saloon horizontal. A detailed description of the apparatus to be employed would, however, occupy more space than we can afford to give to it here.

LIGHT.

Mr. D. S. Holman has contrived a slide for viewing bacteria, vibriones, and other low organisms, under the highest powers of the microscope. The slide consists of a central polished cavity, about which is a similar polished bevel; and from the bevel outwards extends a small cut, the object of which is to afford an abundance of fresh air to the living things within, as well as to relieve the pressure, which shortly would become so great—from the expansion

Fig. 1.



of the liquid within—as to cause the destruction of the cover-glass. No special dimensions are stated for the central cavity. The bevel is usually ⅛th inch in diameter (the engraving is two-thirds of natural size); the small canal is cut through the inner edge of the bevel or annular space outward, for the purpose named above. It is found upon enclosing the animalculæ, &c., that they will invariably seek the edge of the pool in which they are confined, and the bevelled edge permits the observer to take advantage of this disposition, for when beneath it the objects are within range of the higher power object-glasses. Another very important feature in the device is the fact that a preparation may be kept within it for days or weeks together without losing vitality, owing to the simple arrangement for supplying fresh air.

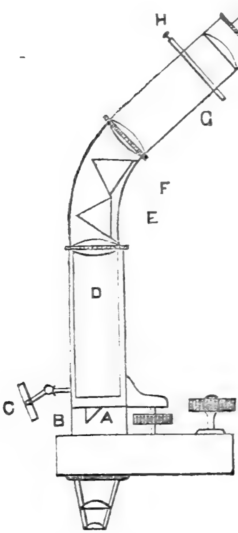
Dr. J. J. Woodward, in some remarks on the resolution of the nineteenth band of Nobert's plate, states that he has obtained the best results with objectives rather under-corrected as to colour. This entirely coincides with the practice of some of the best London opticians who have directed their attention chiefly to the perfect correction of the spherical aberration, and, knowing the impossibility of entirely correcting the chromatic aberration, have always left a small amount of colour, not only without injury to the performance of the combination, but with positive advantage.

Mr. F. H. Wenham has succeeded in constructing objectives with a single posterior as well as anterior lens, the only compound lens being the middle combination. With respect to the fitness of the high angle of aperture of the glasses at present in use for ordinary work, Mr. Wenham considers "that a $\frac{1}{20}$th of about 95°, accurately corrected, and having a long working distance, say $\frac{1}{30}$th of an inch, would be a valuable glass in the hands of a naturalist, enabling him to *see into* things instead of a mere surface observation of a few diatoms, for the sake of performing the feat of defining the difficult marking of some half-dozen of them,—and this is only what such a glass is at present used for."

Messrs. Powell and Lealand have constructed an objective of $\frac{1}{80}$th of an inch nominal focus; its angular aperture is 160°; the magnifying power is 4000 diameters with the A eye-piece, and it bears the B and C eye-pieces with no other detriment than a slight loss of light; it works well through a cover of 0·003 inch. It was exhibited at a recent meeting of the Quekett Microscopical Club, and, notwithstanding the unfavourable conditions under which it was tried, it showed the Podura scale sharply, without colour and with abundance of light.

At the December meeting of the Royal Microscopical Society Mr. Gayer exhibited a micro-spectroscope of novel construction. The slit is placed in the lower part of the body at about an inch distance above the object-glass; the slit is adjusted by the usual contrivances, and a small right-angled prism, B, and mirror, C, supply the means of obtaining a second spectrum for comparison. The image of the slit is formed by the collimating lens, D, above which are mounted two prisms of 60°, E and F. Attached to the curved tube containing the prisms is the telescope, G, having all necessary arrangements for focussing, and also a micrometer, H. When the object to be examined is so small that there is any doubt as to its image filling the slit, the spectroscope is removed, and the ordinary draw-tube with erector substituted; the slit can then be viewed, and so adjusted as to include the whole of the object. The prisms are of such dispersive power as to distinctly split the D line. The points of novelty are the position of the slit and the employment of a telescope to view the spectrum, which of course allows of variations of magnifying power by changing the eye-piece. Mr. Gayer claims for this micro-spectroscope the advantages of increased light and greater dispersion than in the ordinary direct vision instrument placed over the eye-piece. The latter property is not altogether an unmixed gain, for although dispersive power is invaluable for separating the bright lines of incandescent gases, the conditions required for the work of the micro-spectroscope are very different; the majority of absorption-bands are by no means sharp or well-defined at their edges, and are, as a rule, best seen with prisms of comparatively low dispersion, as more powerful instruments only thin out the bands and render their boundaries less evident.

FIG. 2.



HEAT.

Prof. Volpicelli, in "Poggendorff's Annalen," says—It has been asserted that a lowering of temperature is produced when air, which has been compressed in a vessel, is allowed to stream out against the surface of a thermopile. To test this assertion I compressed air in a cylindrical vessel to four atmospheres, and, after the heat of the compression had disappeared, I allowed the air to stream against a thermopile, which was connected with a reflecting galvanometer. Three different results appeared. If the commencement of the air-stream was pretty near the surface of the pile there was elevation of temperature, if it was somewhat distant from the surface the temperature fell, and at a point intermediate there was no change of temperature—the image reflected from the needle was unmoved. These results may also be obtained if air is blown, with an ordinary bellows, against the surface of the pile; only in this case the rise or fall of temperature is less marked, owing to the smaller compression of air. I also obtained the results, though in still less measure, with a centrifugal ventilator. These three results are quite in accordance with the new thermodynamic theory. In the experiments the causes of variation of temperature are of three kinds:—One consists of the destruction of the *vis viva* of the air, or *external* work; a second consists of *internal* work, done by the air molecules which become condensed in the pores of the metal of the pile; and the third of external work, done by the molecules as they expand in their course. The two first cause an elevation, the third a lowering of temperature. It is thus seen how one or other of the three above-described results is produced, according as the effects of the two first causes are greater or smaller than the opposing effect of the third cause, or equal to it. Remove the source of the air-stream and you have, first, a zero point of increase of temperature, then a decrease of temperature. Remove still further, and you come to a zero point of increase of temperature. From this is to be inferred that between these two distances (corresponding to the two zeros) there is a maximum of decrease of temperature which the galvanometer indicates. If it were possible to drive the air against the pile without compressing it, and, therefore, without expansion taking place, the two first causes only would operate and there would be heat produced. But these conditions are unattainable. In order to show to a large audience the transformation of destroyed *vis viva* into heat, I suspended a ball of phosphorus near a wall, and standing about 10 metres off, blew the ball with a pair of bellows against the wall. The ball was set on fire when it struck, not in its passage through the air. In another experiment I let a solid body fall on the surface of the thermopile, the latter being connected with a reflecting galvanometer. The reflected image was then seen to move several degrees over the scale, indicating elevation of temperature. This experiment is quicker and more simple than that sometimes performed in which a body is allowed to fall several times from a certain height on a hard substance, and then applied to the pile.

ELECTRICITY.

Dr. G. Robinson has recently patented a new method of sawing timber. It consists in applying a platinum wire, heated to redness or whiteness by an electric current, to the trees or wood which are to be severed much in the same manner as it has hitherto been employed in removing tumours from the human subject. By fitting the wire with handles so as to be able to guide it in any direction the most intricate fretwork can be cut.

M. J. Jamin has contributed to the French Academy of Sciences a paper, in which he shows that magnetism may be condensed in a manner similar to electricity. Having for some special purposes had a large horseshoe magnet made, consisting of ten laminæ of perfectly homogeneous steel, each weighing ten kilogrammes, he suspended it to a hook attached to a strong beam, and having wound copper wire round each of the legs, which were turned downwards, he put the latter into communication with a battery of fifty Bunsen's

elements, by which means the horseshoe might be magnetised either positively or negatively at pleasure. The variations were indicated by a small horizontal needle situated in the plane of the poles. There was, further, a series of iron plates, which could be separately applied to each of the laminæ, Before attaching any of the latter, the electric current was driven through the apparatus for a few minutes and then interrupted, whereby the magnet acquired its first degree of saturation, marked by a certain deviation of the needle. One of the iron plates (usually called "contacts") was then put on, and it supported a weight of 140 kilogrammes. A second trial was now made; and the current having been passed through again for a few seconds, it was found that the horseshoe would support 300 kilogrammes, instead of 140. The number of contacts being now increased to five, which together in the natural state supported 120 kilogrammes, it was found after the passage of the current that they could support the enormous weight of 680 kilogrammes, which they did for the space of a week. No sooner, however, were the contacts taken off than the horseshoe returned to its usual permanent strength of 140 kilogrammes. This tends to show that magnetism may be condensed like electricity for a short period.

Zöllner has ascribed the electric currents of the earth to the motion of incandescent molten masses beneath the crust, which generate currents in the direction of their motion. He has also stated that all current movements of fluids, especially when in contact with solid bodies, are to some extent accompanied with currents of electricity, which have the same direction as the fluids themselves. To prove this he inserted the ends of the copper wires of a very delicate galvanometer just within the wall of a caoutchouc tube, through which a stream of water was passing; this caused a deflection of several degrees on the galvanometer scale, thereby indicating the existence of an electric current, whose direction was that of the water. The greater the distance between the ends of the wires, which may be replaced by metallic plates, the stronger the deflection of the needle. Beetz, while recently repeating Zöllner's experiment, obtained similar results, but found that the currents have a much simpler origin. The needle is deflected so long as the reservoir in which the water falls is not isolated. The metal, the stream of water, and the reservoir, form a voltaic element, whose current it is that deflects the needle. By filling the reservoir, and dipping the free end of the tube, also filled into it, the current is observed though the water be shut off, nor does any change take place when the tap is opened. By simply inverting the position of the tube, the direction of the current is reversed; this is observed to be the case with or without a flow of water. If the reservoir is isolated, no current is formed, whether the water be allowed to flow or not. When the tap and reservoir are of zinc, no current is produced with or without a flow of water, and with or without isolation of the reservoir. Therefore, according to these observations, no electricity is generated by a stream of water.

TECHNOLOGY.

Professor Chevreul has made a series of experiments on the stability of dyes imparted to silks, damasks, and fabrics used in furnishing, The blue colours produced by indigo are stable; Prussian blue resists moderately the action of air and light, but not of soap; scarlets and carmines produced by cochineal and lac-dye are fast; the most stable yellows on silk are produced by weld.

M. Dubrunfaut, during the siege of Paris, devised an artificial milk, made by dissolving one ounce and a half of sugar in a quart of water, adding an ounce of dry albumen (from white of eggs), and 15 to 30 grains of soda crystals, and then making an emulsion with it by means of from one ounce and a half to two ounces of olive oil. As the war progressed gelatine was substituted for albumen, and slaughterhouse fats, purified by melting at 150°, for the olive oil. One firm made by the latter process 132,000 gallons of milk daily for Paris consumption.

M. E. Daniel states that painting in oil may be executed upon tin-foil spread

out upon a smooth surface, such as glass, the latter having first been moistened to aid the laying out of the tin, and to maintain it in its position. The painting, when dried and varnished, can be rolled up like ordinary paper-hangings, from which it essentially differs in possessing all the variety of tones and colouring that oil paintings admit of. The tin groundwork constitutes a water-proof protection, and, on account of its great flexibility, will follow the various mouldings and contour of the object to be ornamented. To the latter should be applied a hydrofuge mixture; it will then be ready for the decorator. Ordinary gilding may be replaced by this method, as the gold can be applied in the workmanship and the gilt tin fixed afterwards. The advantage of gilt tin over gilding on other metals is, that it is inimical to oxidation; whereas it is known that gilding upon other metals, and notably upon zinc, deteriorates rapidly.

A quantity of tin in ingots was, during a severe frost, sent from Rotterdam to Moscow. On arriving it was found to be in a coarse crystalline powder, which could not be fused into the ordinary condition of tin; for, on the application of heat, it was almost entirely converted into oxide of tin, the appearance of which closely resembled sulphide of molybdenum. On being analysed it was found to contain 99·7 per cent of pure tin, the remainder being lead and iron. The cause of the change was attributed to the long-continued vibration it underwent at so low a temperature. Similar conditions have been known to render wrought-iron extremely brittle, and its texture crystalline and granular.

M. Marion, of Paris, has devised a method of photographic printing; it consists in impregnating paper with ferroprussiate, which renders it sensitive to light. The drawing, which is made on tracing paper, is laid upon the sensitive paper as a negative and exposed to light, after which the sensitive paper is washed in water; the copy is then found to be produced on it in white line on a blue ground, which may be changed to black, the drawing still remaining white by using a tannin solution.

The French Mint has recently coined, for the Bank of France, 6000 or 7000 lbs. of Australian Gold, known as "brittle." All the pieces have been found to be easily broken, and have, therefore, to be re-melted. The defect is attributed to the presence of a small percentage of antimony and arsenic, extremely difficult of removal. These elements are known to produce a similar effect in all metals or alloys that are subject to the molecular changes induced by the pressure and heat developed under the action of the dies in the coining press.

Owing to the fact that water-glass is gradually dissolved out of wood while chloride of zinc is volatile at the temperature at which wood ignites, Dr. Sieburger proposes as a fire-proof paint for woodwork the following:—Two coats of a hot saturated solution of 3 parts alum and 1 part ferrous sulphate are first applied and allowed to dry. The third coat is a dilute solution of ferrous sulphate, into which white potter's clay is stirred until it has the consistency of good water-colours. Another method is to apply hot glue-water as long as it is absorbed into the pores of the wood. A thick coat of boiled glue is then applied, and while fresh is dusted over with a powder composed of 1 part sulphur, 1 part ochre or clay, and 6 parts of ferrous sulphate.

M. Tatro, the inventor of a process for purifying petroleum, states that by adding from 2 to 4 per cent of sulphuric acid, and 4 to 6 per cent of dry lime, agitating the oil with this mixture, and proceeding with the distillation, a larger proportion of burning oil is produced.

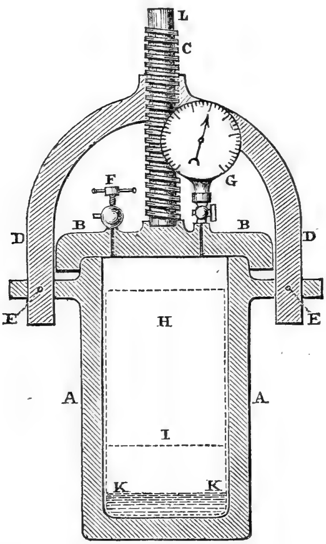
Palmetto leaves have recently been shipped from Savannah to England for the purpose of testing their value in the manufacture of paper.

CHEMICAL SCIENCE.

Having ascertained that furfurol is formed when wood is heated with water to an elevated temperature and pressure, Mr. Greville Williams, F.R.S, explains the method by which he found it to be produced by the action of high-

pressure steam on the same substance. The apparatus employed is shown in the engraving. A A is a bronze autoclave, made in one piece, and of great strength. Before being employed it was tested by means of a hydraulic pump, and was found to withstand a pressure of 500 lbs. to the inch without leakage. Before using the instrument a ring of vulcanised india-rubber was placed between the autoclave and its cover, B B. The screw, C, which serves to keep down the cover, is forced home by means of a wrench applied at L. The arms, D D, serving to support the screw, are affixed to projections on the autoclave, by movable steel pins inserted at E E. A screw-tap, F, enables the product to be distilled over at the conclusion of the operation. The pressures are indicated by the gauge, G. A cylinder of perforated metal, H, is used to contain the substance to be experimented upon; which, in this case, was pine sawdust. The shelf, I, also perforated, prevents contact of the sawdust with the water, the level of the latter being shown at K K. The water and the charge of sawdust having been introduced, the apparatus was immersed to about half its depth in an oil-bath, the temperature being carefully regulated by means of a thermometer. The oil-bath was then heated until the gauge indicated a pressure of 100 lbs. to the inch; this pressure was maintained in some experiments for three, and in others for four hours, the average temperature of the oil-bath being about 198° C. The apparatus having been allowed to cool until the pressure had completely gone down, was then connected with a condensing arrangement. The screw, F, was then loosened, and heat was applied to the oil-bath until about three-fourths of the water present had distilled over. The distillate was strongly acid to test-paper, and smelt decidedly of furfurol, mixed with an empyreumatic odour. On the addition of ammonia it acquired a yellow tint, and in a few hours deposited the characteristic crystals of furfuramide. The crude distillate, mixed with aniline and acetic or hydrochloric acid, instantly gave the magnificent crimson colouration indicative of furfurol. To prove that the crystalline precipitate with ammonia was really furfuramide, this was distilled with a very small quantity of hydrochloric acid; the distillate immediately gave the crimson reaction with aniline. The crystals, treated with acetic acid and aniline, also reacted in the same manner. The author next proceeded to ascertain whether wood would yield furfurol when distilled with water at normal pressures. He therefore distilled 100 lbs. of sawdust with 100 gallons of water, in a still heated by a copper steam coil: 20 gallons were distilled over. These 20 gallons were put into a small copper still, and the first 10 gallons received. These in their turn were rectified again, and two received. In spite of the concentration which these liquors had undergone, furfuramide was obtained on digestion with ammonia, and, in fact, they only contained minute traces of furfurol.

FIG. 3.



THE QUARTERLY
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I. THE COAL-FAMINE.

By Professor EDWARD HULL, M.A. F.R.S.

DEARTH in the midst of plenty. Such is the expression in general use with reference to coal in this year of grace, 1873. In London (as I write) the price is 50 shillings a ton; in Dublin, 40 shillings; in Belfast, something between the two. In various parts of England and Scotland the price ranges from 30 to 50 shillings, according to circumstances. The price may be considered as generally doubled all over the country; and in some districts—situated even on the borders of coal-fields themselves—it is often difficult to procure a ton unless by notice delivered to the coal-merchant several days beforehand. All classes feel the pinch, with the exceptions of colliery proprietors and coal-miners. The wealthy, of course, still keep up their fires, and pay heavily for the luxury; the middle classes, clerks with small salaries, civil-servants, curates, and professional men commencing life, are obliged to stint themselves of warmth, and find that there is much more difficulty in keeping a balance between income and expenditure than heretofore. And the poor — one may well pause to enquire how *they* manage to keep out the winter's frost and cook their little meals while every hundredweight of coal costs two shillings or half-a-crown. Christian philanthropy steps in, and by establishing coal-funds and various means of relief, helps to alleviate the distress; but many a poor widow or worn-out labourer has the ordinary privations of life aggravated fourfold by the want of a good fire — one of the few bright and cheerful things to be seen in a poor man's cottage. We commend this consideration to the attention of that mysterious authority which assumes the right of limiting the supply of coal in order to keep up the price. It may be a supreme source of gratification to have the power of crippling industry, disorganising trade, and causing a severe pressure amongst a "bloated aristocracy," but do the men who pull the wires of this secret organisation ever

reflect that they are bringing misery and want to thousands of poor men's homes? To what extent the increased price of coal is bearing on the resources of the community at large is a question to which Sir W. Armstrong has attempted to give a reply. In a recent address to the North of England Institute of Mining Engineers, he states that the rise in price may be estimated as equivalent to a tax on coal to the extent of 44 millions sterling.* To manufacturers who consume on their works from 100 to 300 tons per week, the difference in price may represent the difference between profit and no profit, or even loss; and already we hear of factories about to be closed and iron furnaces "blown out," while strikes and dear coal have driven many branches of trade away from their original sites.†

Anyone arriving on our shores, and unacquainted with the course of events of the last few years, would naturally conclude that the long-threatened exhaustion of our coal-mines was actually impending; or, at any rate, that the quantity of fuel in the under-ground cellars had become so far diminished that the quantity available for supply had materially fallen off. As a matter of fact, our position is now very much what we should expect it to be if one-half of our available supply was exhausted. If, however, our visitor were informed that the coal is as plentiful as ever, that the difficulties of mining are not materially increased as compared with the last few years, that miners are numerous, and the mines on the whole only a little deeper than when coal was 20 shillings a ton in London, he might well be excused if he received such a statement with incredulity.

And yet such is in reality the case. The researches of the Royal Commissioners on Coal-Supply have fully demonstrated that there is sufficient coal within workable depth to supply the wants of the population of these countries for several centuries, even with an annual increase calculated on the rate of increase of past years. On the basis of a diminishing ratio of increase, Mr. Price Williams, whose views are quoted with approval by the Commissioners,‡ calculates that the annual consumption at the end of a century would amount to 274 millions of tons, and that the total quantity of available coal, as estimated by the Commissioners themselves, would last for 360 years. Another

* Nature, No. 171, p. 271.

† It was recently stated, at a meeting of the Manchester Chamber of Commerce, that the rise in the price of coal may be considered to represent an increase of one halfpenny per pound in the price of cotton. In Lancashire the rise is less severely felt than in some other places.

‡ Report, vol. i., p. xv.

estimate, made on the basis of an arithmetical increase of three millions of tons per annum (the increase of the last fourteen years), would make the consumption at the end of a century amount to 415 millions of tons, and the estimated available quantity would then be only sufficient to last for 276 years. Upon both of these calculations, however, the writer has recently had occasion to remark "that they labour under the defect of not taking into account the diminishing rate at which coal must be consumed when it becomes scarcer and more expensive. The abrupt exhaustion of our coal-fields is an impossibility, and if it is to take place at all it can only be by a slow and gradual process, concomitant with a complete—let us hope a higher and nobler—reorganisation of society." *

Whatever, therefore, may be the ultimate period of exhaustion, it is clear at least that it is far removed from ourselves, and we must therefore look to other causes than that of failure of supply to account for the present high price and scarcity of mineral fuel.

These causes, in our view, are twofold. First and chief, want of thrift and intelligence amongst the mining population; and secondly, interference with the free action of the law of supply and demand. Owing to the former, the miner has generally little desire to emulate the rest of the world in making money, being satisfied if by working short time he can earn sufficient to pay his way; and owing to the latter, the supply of coal is restricted in obedience to the authority of a secret tribunal which few working men have the courage to resist. It might, however, be justly said, that the power of such a tribunal over the individual actions of coal-miners, as of other workmen, is a consequence of want of intelligence on the part of the mining population, —so that the ultimate cause of the present state of things is the low state of education, of thrift, and of self-dependence amongst the working classes. Were the ordinary motives for accumulating money, for "bettering one's self," and rising in the world, prevalent amongst pitmen, and were the laws of supply and demand left to have free play in regulating quantities and prices, it might be assumed that those artificial combinations amongst workmen on the one hand, and employers on the other, which are bearing so disastrously upon the comfort and prosperity of the community, would be unknown.

In order more fully to understand the question, let us

* Coal-Fields of Great Britain, 3rd edit. (1873), p. 454.

briefly review the origin of the present scarcity and high prices of coal.

Upon the cessation of the Franco-German war, when the great duel had been fought out, and the combatants retired within the new boundaries of their territories, a revival of trade brought with it an extraordinary demand for iron. The stocks of pig-iron accumulating in Glasgow and other markets were almost cleared off, and, as a necessary result, the smelting-furnaces all over the country were "blown in," and then sprung up a great demand for coal with which to feed them. The price accordingly went up, and doubtless the proprietors of the mines were the first to feel the benefit of the enhanced prices; but there soon followed, as was perfectly natural, a demand on the part of the miners for increased wages, which was generally acceded to; and ultimately wages increased to such an extent that with restricted time a pitman of ordinary skill can earn at a rate varying from £120 to £150 per annum, and, if he condescends to work five days in the week, considerably more.

A reaction has, however, set in; the enhanced price of iron has shortened the demand, and with this ought to come, in the ordinary course of things, a lessening of the demand for coal and a fall in prices. When notices of a reduction of wages were served on the pitmen, the result has invariably been to cause a strike, such as that we have just witnessed on a gigantic scale in South Wales. The price of coal has not fallen, as was to have been expected, for the pitmen have been taught by their leaders that the price may be artificially kept up by shortening the time of labour and restricting the supply. The miner has learned that by working four days in the week he can earn enough wages to supply his wants for the seven, and he does not care to earn more. The idea of "making hay while the sun shines," of laying by money earned by working the ordinary time allotted to mortals for work, is not one by which he is governed,—or, if so, he is prevented from acting upon it by a mighty unseen influence to which he feels bound to render unquestioned submission.

If the colliery proprietors insist on a reduction of wages, or longer hours of work, the result is a "strike." Looked at from a neutral stand-point, it is impossible to conceive a more clumsy device for settling a question between employer and employed, especially in coal-mining. For it is abundantly evident that, in the vast majority of cases, if the proprietors of collieries could see their way to a fair profit, by yielding to the demands of the men, they would do so

sooner than expose themselves to the disastrous consequences of a general strike over a large mining district. For let us enquire for a moment what are the consequences to both parties in such a district, for instance, as that of South Wales. To the employer it means loss of customers, cessation of interest on capital invested in the mines, often very large, deterioration of plant and machinery, the mines becoming choked, or becoming filled with water in some instances; and, lastly, the spectacle—which to a man of even ordinary humanity must be hard to endure—of destitution and misery around his own doors, or at least on his own property. To the employed a strike means either a miserable pittance doled out from some Union Fund,—instead of abundant wages,—the exhaustion of the store laid by for "a rainy day," or starvation itself. It means idleness in place of industry, poverty instead of wealth, degradation and demoralisation instead of self-respect. And when all is over, when the war has been waged "to the bitter end," the workman returns to his employment morally and physically impaired; and often, after the loss of a considerable sum in hard cash, commences again with wages no higher than those against which he struck.

Mayhap the result of a strike is to annihilate some branch of manufacture, or to drive it from the district; and the workman finds, when too late, that he has been taking the bread out of the mouth of himself and his family. The ship-building trade of London is a case in point; and in South Wales, where iron-smelting was in some cases a source of little or no profit to the employer, the result of the recent strike has been to close, perhaps permanently, a considerable number of iron-furnaces, whereby a large number of men will lose their daily bread.

If these views were more generally understood amongst the mining population, and if they would exercise that independence of thought and action which is the heritage of every free man, strikes would become a thing of the past; men would work, and the price of each commodity would find its own level according to the laws of political economy. It is to be feared, however, that the mining population is in a state as regards education which is not creditable to a British subject. In some districts, both in Scotland and England, the miners and their families are in a state of gross ignorance, and so wretchedly housed that even decency is out of the question. This may be due, in some measure, to their improvident habits, for the wages they earn are sufficient to provide them with much better accommodation.

But it is also a matter which the employers should look after; and we venture to think that with the inducements offered by a substantial house, with good accommodation for a family, habits of temperance and forethought would be nourished. In some cases suitable residences have been provided, and with good results; and I now have before my mind, in a central county of England, the recollection of a substantial row of collier's houses, each with a little garden in front, and with four—or at least three—rooms for the tenant; and not only have they been let to the colliers at fair rentals, but the manager of the works takes good care that they are properly used, and kept in order by the inmates.

Much remains to be done to improve the condition of the working miner; but while he remains often in a state unworthy of a Christian community, can it be wondered at that he should be a ready instrument in the hands of designing men, and surrender the right of private judgment and individual action to self-appointed leaders as ignorant as himself, and far more selfish?

One of the main causes of the present short supply of coal is the refusal of the miners to work full time. Their fathers were accustomed to work five, or even six, days in the week, but the present generation is content with four or four and a half. In consequence of this the mines are unoccupied during two and a half or three days in the week, less coal is raised, the price is advanced—owing both to the short supply and because the proprietor has to recoup himself for the absence of return on his capital during the idle days.

The absence of a desire to accumulate money, the reverse of which may be regarded as in some measure an evidence of civilisation, so general amongst other classes, is a peculiar feature in the case of the miner. Most of us are willing to do extra work in order to add a few pounds a year to our incomes; but with the pitmen of parts of Lancashire, Staffordshire, and Scotland, the case is otherwise. The old motto, "A fair day's wage for a fair day's work," has given place to a new one, "The least amount of work for the largest amount of pay." Unfortunately the time left at the miner's disposal, owing to the short system, is not generally turned to any very high purpose; so that the high wages and short time is of little benefit to the miner himself, and is most injurious to the community at large.

Until, however, the miners can be induced to work during

a reasonable time, and supply us with sufficient coal, it behoves the employers to render themselves as independent as possible of their services. This is only to be done by extensive introduction of mechanical contrivances for doing what cannot otherwise be done by hand. Coal-proprietors and managers have been probably too slow in making use of such inventions, and should not delay in turning to account the present state of the labour market. There are many coal-cutting machines invented, all of more or less use in saving human labour; one of these, worked by compressed air, the patent of Messrs. Baird and Co., of Gartsherrie, is stated to be capable of doing the work of forty men, and requires only four men to manage. Supposing this to be so, and as there are about 360,000 men engaged in coal-mining, it is easy to see that, with the universal use of such machines, 9000, or say 10,000, men would be sufficient to turn out the 112 millions of tons, now sufficient for our wants. Of course, from various circumstances, this could not actually be carried out; but the extensive introduction of such machines, by throwing a large number of unemployed hands on the labour market, would have the effect of bringing down wages within reasonable limits, reducing the price of coal, and making it abundant.

The rise in the price of coal cannot, however, be altogether attributed to the action of those engaged in actual mining either as employers or workmen. While the price in London is 50 shillings a ton, it is stated that at the pit's mouth the price is only 20 shillings, while the railway charge is only 6 or 8 shillings. After making all reasonable allowances for cartage and profit by merchants, it ought not to be much over 32 shillings to the consumer. If this be so, the profit to the middle-men or merchants must be beyond all reason, and the public must have recourse to self-protection. This can be done by co-operation, which has worked so advantageously in other departments of produce. Let companies of private individuals be formed who will enter into agreement to deal directly with the coal-proprietor at the pit's mouth, and supply themselves and the public at moderate rates. The undertaking will be attended with no greater difficulties than those already overcome by co-operative societies. We are glad to see that proposals for such co-operative societies for coal supply have actually been made.

It cannot, however, be said that the public has not in some degree merited the pressure it now feels. The waste of coal in manufacturing and household uses has been

beyond bounds, and required some sharp remedy in order to work a cure. The only branch of industry where the consumption of coal has been reduced nearly to a minimum is in iron-smelting, and this only in some special districts, such as those of Middlesborough, North Lancashire, and parts of Scotland. What can be done by improvements in the way of economy is curiously illustrated by the history of iron-smelting. At the Clyde iron-works, in 1796, according to the account of Mr. Mushet, no less than $9\frac{1}{2}$ tons of coal were consumed in producing 1 ton of pig-iron. The quantity of coal now consumed has been reduced to 1 ton $14\frac{1}{2}$ cwts. with the hot blast, or 2 tons 3 qrs. of coke. In the Middlesborough district, where the expenditure of fuel has been reduced to a minimum, the quantity of coke and coal combined amounts to 33 cwts. 1 qr. to the ton of pig-iron. In this district the hot air and gases escaping from the throat of the furnace are used for calcining the ore, heating the blast, and generating the steam for driving the blowing-engine.

In steam navigation a much-needed saving is being rapidly effected by the introduction of double cylinders; the first working at high-pressure, the second using the steam over again in conjunction with a condenser. This system has been in use in France for the last twenty years (as I am assured by Prof. O'Reilly), and is now used in all the ocean steamers of modern construction. The saving of fuel may be taken at not less than 25 per cent, and to this advantage there is to be added the important one of additional storage room for goods.

The greatest amount of waste lies in household consumption. The British public seems inveterately wedded to large blazing fires, so constructed as to send three-fourths of the heat up the chimney. Until we overcome our prejudices in favour of the present form of fire-grate, no large amount of saving can be effected; but, unquestionably, some modification combining the heating surfaces of the stove with the cheerfulness and ventilation which are the chief advantages of the present form of open fire-grate would be the means of effecting a large amount of saving in house-fuel. Mr. R. Hunt—our best authority on this subject—considers that the amount of coal consumed for domestic use may be taken at 1 ton per head of the population, and that about one-third of the whole quantity raised is thus consumed,—that is, about 37,000,000 tons. It is probable that the general substitution of stoves, or of such a combination of a stove and grate as above recommended, would result in saving

one-third of the above quantity, or twelve millions of tons—a quantity nearly equal to the total export of coal from British ports.

It cannot be supposed that such a social revolution as the re-construction of our house fire-grates involves will be immediately accomplished; but the foregoing statements will be sufficient to show how much lies within our power, both in the way of increasing the output of coal, with diminished cost at the mines, and of economising the domestic supply without the sacrifice of warmth within doors. To the co-operation of the colliery proprietors on the one hand, and of the public on the other, we must look for an increase of supply and a reduction in the demand; the former by the extensive increase of machinery, where hand-labour is now in use; the latter by the introduction of grates constructed with a view to economy. To manufacturers, who are always alive to the principle that "A penny saved is a penny gained," we may trust to avail themselves of every improvement that offers itself in the direction of economy in fuel.* And with regard to the miner, let us hope that the measures which the Legislature have recently passed for securing to the young a sound education may have the effect of rendering the rising generation more industrious, more thrifty, and more independent of influences from without in those matters of which every man should be the sole judge for himself.

II. RAILWAYS AND THEIR FUTURE DEVELOPMENT.

By J. W. GROVER, Memb. Inst. C.E., &c.

IT seems a very hard dispensation, though it is an incontrovertible one, that those who have, perhaps, conferred more benefit upon the country during the present century than any of their contemporaries, should reap so little of the reward themselves. The railway shareholder—I mean the original man who honestly read the prospectus, and believed in its statements, and who backed his belief with his money—was too often a victim to his credulity and enterprising spirit.

* The statement of the Royal Commissioners on this head is satisfactory. While admitting that coal is still wasted largely in consumption, they add that for some time past, in our manufactures, there have been constant and persevering efforts to economise coal by the application of improved appliances for its consumption. Report, vol. i., p. 93.

Yet, why not? all venturers stand a risk. Certainly, mines are more sporting investments, to say nothing of the wonderful and fearful enterprises in unknown corners of the American continent, into which the British public plunge with a confidence worthy a better cause. To these, at least, there is a hope of some sort, remote though it be; the story may be true—diamonds may be found in ant hills—and a good round bonus be the occasional reward of the speculator. But the unfortunate railway shareholder has no such hope; if, after years of earnest expectation, he reaches the grand consummation, the *summum bonum* of five per cent., he is thankful if not satisfied.

Hence, few will embark in fresh railway enterprises legitimately; and this being the case—as it undoubtedly is—we may conclude the summit has been reached. It is true the rivalries of contending companies will induce them to support branch lines, but in themselves these branches are suckers rather than feeders—justly regarded as necessary evils—to be tolerated only where they cannot be avoided, as Dr. Johnson said of notes in books. It is now just four years ago since the Chairman of the London and Brighton Railway Company told his proprietary, who had subscribed four millions towards the construction of a number of branches, that they might as well have used the bank-notes to light their pipes with; therefore, several important authorised lines for which the land had been actually purchased, and, indeed, the works partially completed, were abandoned, to the chagrin of the districts they were intended to serve.

Various attempts have since been made, both in Sussex and Kent, to revive these defunct undertakings, hitherto without success, and as the system now stands, the failures are likely to be repeated, and even success itself promises a crop of financial burthen and disaster.

There is a want of something different from what has gone before, and several engineering gentlemen of eminence have given us their ideas on the subject; the gauge question has been revived by Mr. Fairlie; Mr. Fell has brought out the central rail invention, and others equally novel and ingenious; wire tramways, as they are called, have been built for the conveyance of minerals, and suspended railways for the conveyance of passengers on the tops of posts have been proposed by one eminent advocate. Yet still no practical progress has been made, and we find ourselves where we were when we began.

Now, it is necessary to begin at the beginning, and to

consider the very elementary principles of a railway's existence, to look at the physical and financial questions fairly, and having them before us, to settle what is to be done in the future: for depend upon it, the less we ignore the teachings of the past the better; there is no sound progress apart from experience; hence it is that reforms are seldom introduced from without, although it is the external pressure which causes them.

It will be well to deal with the physical questions first of all, before entering upon the financial. The primary conception of a railway is a perfectly smooth, level, and straight road, upon which friction is reduced to the minimum, so that heavy loads may be propelled with the least possible resistance, and at the highest rate of speed.

The earliest type of locomotive engine was designed to run upon such straight and level roads, and it was supposed for many years that locomotives could not climb hills or be made to go round corners.

The first railway carriages were a simple modification of the stage coaches, names and all. It is interesting to look at the curious three-bodied "*Marquis of Stafford*,"—with yellow pannels and windows, filled with ladies in large coal-scuttle bonnets—as shown in one of Ackermann's early engravings of the Liverpool and Manchester Railway, the only substantial difference being that, inasmuch as the railways of those days were made nearly straight, no arrangement was provided for allowing the axles of the carriage to radiate as they do partially in common road vehicles, but both axles were rigidly fastened so as to be immovable.

Again, as all road vehicles have to turn abrupt corners, their wheels are made to turn independently upon their axles, but so soon as flanges were employed to keep the wheels of the railway carriages between two straight rails, this arrangement was found unnecessary, and to obtain greater strength and security, the wheels were rigidly fastened to the axle, and both were compelled to revolve together.

Now, since the primary conception of the perfectly smooth straight road, a great degeneracy has been of necessity taking place; with greatly increased demands, less capital than ever has been forthcoming; consequently the great cuttings and embankments of early days are being abandoned as precedents, and it becomes necessary that railways should approach more closely to the form of ordinary roads, which follow the surface of the ground only—at small cost.

Hence it follows that the rolling-stock itself must revert more nearly to its original pattern, readopting those contrivances which, under altered circumstances, were discarded.

Still keeping to the most elementary principles, for it is these which are forgotten and misunderstood, and yet they should be engraven on brass and hung up in every railway board room in the world. On a common road, a horse can pull a ton weight in a cart behind him on the level at 4 to $4\frac{1}{4}$ miles an hour, or, which is the same thing, if a weight of 70 lbs. were hung over a pulley and lowered down a well, he could pull it up at the speed mentioned. It is necessary to be a little explicit, as the remarks in this paper are intended for non-technical readers particularly. Now if two strips of iron called rails are laid upon the aforesaid road, the friction is reduced seven-fold, that is to say, the same horse at the same speed could draw 7 tons, the difference between macadam and iron being as 70 lbs. to 10 lbs. This immense advantage, however, disappears when gradients have to be encountered, because the resistance due to gravity becomes so greatly in excess of the resistance due to friction, and is constant in both cases. For instance, if on a common road, up a slope of one foot in ten, the horse takes 5 cwts. in a cart over the macadam, if rails be laid down up the same hill, he could only increase the burthen behind him by a little more than 1 cwt., or, in all, $6\frac{1}{4}$ cwts.; hence, in this case, the value of the rails is nearly lost. Hence the small use of tramways where hills occur.

Upon a very good macadamised road the resistance due to friction is usually taken at about *one-thirtieth* of the whole load carried; that is to say, if the vehicle were put upon a road sloping 1 in 30 it would just begin to move of itself. But upon a railway, under the most favourable conditions, the resistance due to friction has been reduced to the *two-hundred-and-eightieth* part of the whole load carried; that is to say, the vehicle will begin to move of itself on a gradient of 1 in 280. In considering the work which a horse can perform on a tramway, it is important to bear in mind the question of speed; for, according to the experiments of Tredgold, he can draw exactly four times as much at two miles an hour as he can at five, and it appears that at three miles an hour he does the greatest amount of actual useful work, whereas at ten miles an hour only one-fourth of his actual power is available, and he cannot exert that for an hour and a half; whereas at two and a-half miles an hour he can continue

working for eight hours. Having these data before us, it is easy to compare the values of steam and horse-flesh:—Suppose coals to cost in the midland districts 18s. 8d. a ton only, or one-tenth of a penny per lb., and assuming that an average locomotive engine will not consume more than 5 lbs. of coal in the hour per horse-power, the cost of fuel per horse-power will be a halfpenny per hour. Taking the value of the horse's provender at 1s. 9d. a day only, and supposing he works for six hours, that would cost $3\frac{1}{2}$d. an hour against a halfpenny in the case of steam, or, as 7 to 1 in favour of steam; and this result is obtained on the supposition that the horse travels only at three miles an hour.

Now, to sum up the combined advantages, therefore, of an engine on a level railway against a horse on a level common road at 10 miles an hour, we shall find that the former gives an economy over the latter of nearly 300 to 1; at 5 miles an hour it would stand as 115 to 1; and at $2\frac{1}{2}$ miles an hour as 64 to 1.

Such are the enormous advantages of *steam and rails*, and with them does it not seem astonishing that better financial results have not been obtained? There must be something wrong somewhere. As Artemus Ward says, "Why is this thus, and what is the reason of this thusness?"

Speed is the delinquent, and the cause of the loss of the great primary advantages: the vehicles on railways are propelled very fast; hence they involve great strength in their construction, and enormous weight in proportion to the paying load carried.

An old stage coach, according to Nicholas Wood, weighed only 16 to 18 cwts., and would carry upwards of 2 tons of paying passengers with their luggage, or about $\frac{4}{10}$ths of a hundredweight of dead load to every hundredweight of paying load. Now, a third-class carriage with four compartments would represent 2·8 cwts. of dead weight to every 1 cwt. of paying load. Therefore the stage coach has the advantage over the third-class railway carriage of $6\frac{1}{2}$ to 1.

It becomes impossible to institute any absolute comparison between roads and railways at speeds above 10 miles an hour, because such speeds are impossible on the former for any considerable distance. Again, the question of gradient has to be noticed, for in the preceding remarks a level road and a level railway have only been considered.

As has been explained, where steep gradients occur, the resistance due to gravity so much outweighs that due to friction that rails afford a comparatively insignificant

advantage, and one which is entirely lost if the stock has to be increased in weight $6\frac{1}{2}$ times.

It may easily be shown that on a gradient of 1 in 10, for instance, taking the foregoing figures, that the advantages of a steam-worked railway over a horse-worked road would be a little more than one-fourth, if the stock on the former be only $6\frac{1}{2}$ times heavier in proportion than the latter would require. Hence it follows that no railway having gradients of 1 in 10 could be worth making (assuming such to be possible) unless the stock upon it were assimilated to that of the ordinary omnibus or stage coach-type.

In former times calculations were made by Nicholas Wood of the comparative costs of conveyance on ordinary roads by horses; he showed that on an average a stage waggon could carry at the rate of $2\frac{1}{2}$ miles an hour profitably at 8d. a ton per mile; that a light van or cart at 4 miles an hour could take for 1s. a mile a ton of goods. Passengers in stage coaches were charged 3d. a mile each, or 3s. 6d. a ton, at 9 miles an hour. Now let us consider what railways actually do. At the present moment coals are conveyed at 5-8d. per ton per mile, at an average speed of 20 miles an hour; and this low rate actually leaves a profit. Excursion trains take passengers at less than $\frac{1}{2}$d. each per mile, at 20 miles an hour, or at 7d. a ton a mile.

Now, bearing in mind the relative proportions of paying and non-paying loads involved in carrying passengers and coals, a simple calculation will show that a ton of passengers could be carried for something less than 1d. a mile, or $\frac{1}{14}$th part of a penny each. For, although passengers require station accommodation, they unload themselves, which coals do not.

In the autumn of 1869, the "Times" took up the railway problem, and in a series of very able articles endeavoured to show the errors of the present state of things. Although advocated by so powerful a pen, the reforms still remain unaccomplished—indeed, uncommenced. It was then shown that in practice every passenger on a railway involved over 2 tons—of iron and timber—to carry him. Or, according to Mr. Haughton (late of the L. & N. W. Railway), no more than 30 per cent of the load which is hauled by a goods train represents paying weight, the remaining 70 per cent being dead weight. This seems astonishing, truly, but it is nothing to the passenger trains, where only 5 per cent, or even less, of the load pays, the remaining 95 per cent being made up of apparently dead and unprofitable material. It is well to keep this clearly in view. In talking about a passenger, with relation to a railway, one must not picture

to oneself a respectable English country gentleman, riding, perhaps, some 14 stone, but some Homeric giant, magnified into prehistoric proportions, weightier than an ordinary Ceylonese elephant, and representing about 20 to 25 full sacks of coals, or $2\frac{1}{4}$ tons.

Yet for three years and more these "facts" have been made manifest, and nothing whatever has been done; and, as matters stand, no alteration of any appreciable extent is possible, or else it would have been effected long ago. High speeds involve high requirements and great strengths in under-frames, in buffers, couplings, axles, and the entire fabric of the vehicle, besides in the engine, demanding large fire boxes and driving wheels. If trains are to run at 60 miles an hour their construction cannot be materially altered without some change in the general system itself. Thus speaks the oracle:—"The railways of the United Kingdom are conducted by an accomplished, scientific, and highly-skilled body of experts, who know their business, do it, and don't talk about it; and who, moreover, take out of the locomotive all they can, and present it freely and *exuberantly* to those whom it is their interest as well as their pleasure to accommodate—the travelling community."

These remarks are but too true; the travelling community has been well cared for; perhaps the unfortunate shareholders in future undertakings should be accommodated too —by a slice in what is to be so freely and "exuberantly" given away to those who have taken no risk in the venture.

Let us proceed to dissect the existing state of affairs financially, and see where the money goes, and how. Perhaps the last year or two have been exceptional; we will take three years ago. Out of every £100 earned £49 have to be paid away in working expenses, leaving £51 to be divided amongst those who built the line.

How are those £49 spent? The table at top of next page will show generally.

The first three items vary according to the rate of speed employed; they form more than one-half of the whole costs, or 54·54 per cent. A very moderate computation would show that if lower speeds were employed, not only could the stock and engines be reduced in weight, but the wear and tear would be considerably mitigated; the 54·54 per cent would be reduced to somewhere about 36 per cent, or 18 per cent less, increasing the available balance for dividend from 51 to 69 per cent, or from 5 to nearly 7 per cent.

	Per cent of Working Expenses.
1. The maintenance of the way and works costs	18·43
2. The locomotive powers	27·93
3. Repairs and renewals of carriages and waggons.	8·18
4. Traffic charges (coaching and merchandise)	28·72
5. Rates and taxes.	4·02
6. Government duty	2·47
7. Compensation for personal injury . . .	1·63
8. Ditto loss and damage for goods	0·95
9. Legal and Parliamentary.	1·44
10. Miscellaneous expenses	6·23
Total.	100·00

To put the case more simply, suppose a train earns on an average 5s. 2d. per mile, the working expenses would be 2s. 6d. a mile, made up in the following way:—

	d.
1. Maintenance of way and works	5·53
2. Locomotive powers	8·38
3. Repairs and renewals of carriages and waggons	2·45
4. Traffic charges (coaching and merchandise)	8·61
5. Rates and taxes	1·20
6. Government duty.	0·74
7. Compensation for personal injury . . .	0·49
8. Ditto loss and damage of goods .	0·29
9. Legal and Parliamentary	0·43
10. Miscellaneous expenses	1·88
Total working expenses . . .	30*d.*

These figures are the A B C of the railway system in England as it now exists, and supposed to be the most perfect in the world, so far as comfort, speed, and constructive skill is concerned; and the most unsatisfactory as far as commercial result goes, returning on the actual outlay little over 4 per cent. It ends in this, practically, that on the most perfectly smooth surface a train costs 2s. 6d. a mile to run it, to carry an average of 70 passengers, thus showing an average of nearly 6d. a ton a mile. As the system now stands nothing better can be hoped for: competition compels

extravagance and destructive speeds; and, furthermore, the travelling public have been so spoilt by the useless waste of space in the three classes, with smoking and non-smoking division, that any attempt at reform would be vigorously and successfully opposed.

The present enormous weight of dead load to paying load in England is to be greatly accounted for by the variety of classes and the fluctuating demands for accommodation; for to each class there must be a large margin of allowance.

We have—

First Class.
Ditto Smoking.
Second Class.
Ditto Smoking.
Third Class.
Break van and engine.

Here we have five different sets of travellers to accommodate; and sometimes, as on market days, there will be three times as many persons of one particular class to accommodate as on others; therefore, practically, on each of the five orders nearly treble the average demand must be provided for. It is all very well for main lines, but on branches something else is requisite. Let there be but two classes—

1. Covered carriages, no smoking.
2. Open side cars, smoking.

And by the use of continuous breaks safety can be increased and a break van dispensed with.

We should here have two classes instead of five, and, therefore, bearing in mind that three times the average number carried has to be allowed for, a proportion of six to fifteen in our favour. It cannot be too often repeated that what exists cannot well be altered; the public have acquired certain rights by mere custom, and they must be maintained; but it is in view of future undertakings only, that the terms of the new contract can be revised, as between the public and the coming shareholder. It is, after all, the public's best interest to do away with that which impedes railway development, for it is the public who reap the advantage.

When a little branch railway has to be constructed, why should the country expect a scale of magnificence in works and stations like that upon the main line from London to Liverpool; why should the undertaking be saddled with bankruptcy from its inception, and what is beneficial in itself be converted into a bye-word and a hissing. The fact is, that the world, not excepting engineers themselves, has

been educated up to a certain standard of requirements, and hence it is absolutely hopeless to look for any change in England in "Railways." Like the Circumlocution Office, or a Government department, or one of those old-fashioned blowing engines which I have seen in the iron districts, which does its work, and must not be meddled with, or else it would stop altogether, the "machinery" would get out of order by interference, and once out, it could not be readjusted.

A railway is a railway, and you cannot make anything else of it. A "light railway" is a misnomer—a term which has led to a great deal of confusion and loss of money, although it has received the sanction of the Legislature (31 and 32 Victoriæ)—a "light" railway must be a bad railway; therefore it is as well to descend at once from the lofty eminence, and talk about a tramway, *steam worked,* if you will, but still a "tramway," and not a railway; then at once we begin to approach the region of dividend and commercial prosperity, and the investing public can be once more appealed to with prospect of success, and we work on a different scale and without that majesty of design, which must end in *disaster* and disappointment.

Before approaching the practical part of this paper, and showing what really ought to be, instead of what ought not, I should briefly draw attention to the fact of a "light railway" in this country being almost an impossibility—not physically, but from the surroundings. I speak from experience: a branch railway is projected on the ordinary system, and receives the sanction of Parliament; a great deal of difficulty is found in raising the money, as nobody will subscribe who can help it; a director or country gentleman, who promises a thousand pounds or two, does so simply out of patriotic devotion to his district—for its development—and looks upon the money as a *fonde perdu,* irretrievably gone. The town to be benefited is canvassed by a few enthusiastic agents, who succeed in placing a few hundred shares of £5 each amongst the tradesmen, who give as they would to a charitable association. At last it is found that the whole amount got together is infinitely below what is wanted; indeed, only a fractional part of it.

Then an appeal is made to the Board of Trade, to give permission for a reduction in the style of construction,—light rails, light permanent way, light bridges, light stations, all cheap and bad, and in the end most costly—are sanctioned, and twenty-five per cent is knocked off the required capital. All promises well; inspired with fresh confidence,

the directors venture on a start, and something begins to show in the country. Then comes the fatal step, the trunk line, which the branch runs out of, never having had any confidence in the little sucker, and having treated it with contempt, if not with hostility, begins to see an actual move, and therefore undertakes to work the line at 50 per cent, perhaps, of the gross receipts. All goes well now, the 50 per cent agreement is what everybody has been crying out for, and at last have got, but it is a new era of misfortune only—the reign of King Stork over King Log.

The working company, before taking over the new property, instructs its engineer to report upon its condition; he is a gentleman who has been used to the substantial abundance of the past; he does not understand the "light" system; to him a light rail is a bad one. His engines weigh 45 tons with their tenders; and he knows the locomotive superintendent will pick out one of the oldest and worst to work this unfortunate branch, besides a few old coaches unfit for the main line, therefore he cannot accept light bridges. Again, he will find that all the gradients have been made steep, and the curves sharp, to avoid expensive earth works; this in his opinion, and justly, would actually involve a heavier permanent way than he is using on his main line, and so on, till the whole thing has to be re-made; and the working expenses—nominally 50 *per cent*, but, in effect, with all sorts of junction charges and renewal claims, over 65 per cent—entirely swamps the "light system," and its specious and delusive economy.

Wise and able men amongst engineers have seen and felt this, and have freely acknowledged that a branch line must be absolutely something *different* from the parent stem, so that it could not be worked in common with and *into it*. Hence, they have advocated change of *gauge*, apart from its own intrinsic merits, as most completely defining the two systems and preventing their overlapping; it certainly does give to the smaller system an independence and integrity which has great advantages in many ways, but the isolation is too complete in a small country like England, already intersected with lines of a generally standard gauge, except in one or two instances, and these especial. For the undeveloped States of Europe and America, for South America and our Indian Empire, where distances are vast and traffic sparse, a gauge narrower than 4 feet $8\frac{1}{2}$ inches can be used with some advantage and economy; and if the country is at all rough or mountainous, with a mineral traffic, then the necessity for the small gauge is paramount, for it then becomes a question of small gauge against no line at all.

The little Festiniog Railway, in North Wales, has been frequently illustrated in support of the arguments for an extremely narrow line, for though only 2 feet wide between the rails it has paid dividends exceeding 12 per cent—that it has been assumed somewhat hastily that the dividend varies inversely as the gauge, and that by halving the width between the rails the profits can be doubled. The fallacy of this argument is proved at Festiniog itself; for there, even on the face of the same grand mountains, overlooking the same fair valley of the Dwryd river, is another line, not a branch of the first, but rather its continuation to the village of Festiniog, though worked and made by an independent company, which has returned no dividend to its shareholders.

The Festiniog Railway proper has great advantages quite exceptional, and these have been turned to the very best account by the skill and energy of Mr. C. Spooner, C.E., the engineer, who, by adopting the Fairlie Double Bogie Engines, has obtained great power under very adverse circumstances and want of room. Yet it must never be forgotten that the elements of success are manifest. Over one hundred thousand tons of slate annually have to be transported, and all down hill: there is not a fifth of the load to take back in the empties; there is no competition whatever. The toll has been nearly treble, at least over double that charged by any other line for many years; and the line has actual agreements with most of the great quarries by which they would be prevented from any independent action to reduce their freights.

The slates in themselves form a most compact and handy class of goods for carriage. The average speed, moreover, of passenger and goods trains does not exceed 8 to 12 miles an hour. All these circumstances prevent us from taking the Festiniog line as any fair example of a system which would work well elsewhere. To visit it, and to enjoy one of the delightful rides up the mountain side, with the panorama of land and sea around and crags above, and look down on the meadows by Maentwrog spread out as a verdant *parterre*, severed by the silver riband-like stream, is a pleasure to be remembered in a life. There is no such thing to be found elsewhere in the wide world; it is unique, and the enjoyment is accompanied by the exquisite sense of having made some new discovery. Let us for a moment analyse the feeling of having unlearnt the great railway lesson one has been learning all one's life till one visited by chance the vale of Festiniog. For this, and this alone, the journey is worth making, and whoever goes with

his eyes open will not return empty; he will feel that his preconceived notions of what was necessary to a railway's existence are torn to shreds and scattered to the four winds. His prejudices of railway education will have been shaken to their foundation, if not uprooted altogether, and he will say with the philosopher of the last century, that "all his knowledge only shows him that he really knows nothing at all."

Hence the great success of this Festiniog Railway as an exemplar. Because it is different it has been taken hastily as perfection, and has been recommended in cases to which it is wholly unsuited. Yet honour to it for its great work. The Russian Empire, the North and Southern Continents of America, and now India itself, have not thought it beneath them to learn from the little Welsh Railway; and it may be truly said that it is the first practical step in the right direction, and has awakened men's minds more than anything else to the necessity for something different, and something better.

It will now be the object of this paper to describe a small and very unpretending "steam tramway," constructed by the Duke of Buckingham for the development of his properties in Buckinghamshire, which in the writer's opinion seems to offer the most universally applicable example of what branch railways must be in the future in England, and perhaps in less developed regions of the world's surface.

This little line was commenced on 8th September, 1870, and the first four miles, from Quainton as far as Wotton, were opened on 4th April, 1871; the greater portion of the remainder was used for mineral and agricultural produce in November, 1871, but the last quarter of a mile up to Brill was not brought into use till April, 1872. The main line is nearly seven miles long, and the gauge the same as that upon the adjoining railways, viz., 4 feet 8½ inches.

The cost of this "steam tramway," including sidings and two goods sheds, was rather less than £1400 a mile without land, which belongs principally to the Duke of Buckingham. The gradients between Quainton and Wotton are favourable, the worst being 1 in 78. But from Wotton to Brill they are comparatively heavy, varying from 1 in 100 to 1 in 51, the total ascent in the last three miles being 130 feet. The line is worked by Messrs. Chaplin and Horne, but the maintenance is undertaken by His Grace the Duke, who executed the work with the assistance of his own engineers, and without a contractor. The expenses of maintenance (and certain other works) is at the rate of

£380 a year; the total working expenses being estimated at £650, including 10 per cent interest on two engines; the earnings being at the rate of about £1350 to £1400 a year, leaving a profit for dividend at the rate of over 7 per cent on the outlay, exclusive of land in the first year, a result probably without parallel in the history of English railways.

This little line traverses the most ordinary agricultural country; there are no great slate quarries or manufacturing establishments to create any exceptional trade. The case is one which affords a striking instance of what can be done by steam and rails in common English country. The traffic consists of coal, road metal, manure from London, and general goods inwards; of hay and straw, grain, timber, bark outwards; of cattle inwards from Herefordshire in spring, and fat cattle to London in the winter.

The coaching traffic consists of passengers and milk, at present carried by a Great Western composite carriage, which has been borrowed, and which weighs 8 tons, a great deal too much for the work it has to do.

The line is worked by one 6-horse Aveling and Porter engine, weighing less than 10 tons, and costing about £400. The engine makes two double trips a day, and a second one is now provided. The former is found sufficient, and very low rates are charged, London manures being brought at 1d. per ton per mile throughout.

It should be observed that these engines have no springs, and consequently travel somewhat roughly. Perhaps too great economy has been sought in them; an expenditure of £600 would have ensured a really efficient machine.

The speed employed varies from four to eight miles an hour, and it was not intended to carry passengers at all in the first instance; but the demand for accommodation in the trains was so great that the passenger carriage had to be borrowed, and the numbers carried were 627 in the first four months of last year.

Unfortunately, no statements of the actual costs and earnings are published beyond April last; but the impulse to trade and agriculture, due to the tramway, is extraordinary; and has exceeded the best expectations. The district served is one by no means densely populated, on the contrary, the whole of the villages, including Brill, do not total up to more than 2000 persons, or less than 300 a mile. Three years ago, the idea of making a branch railway to serve such a district would have been considered insanity; for all over the country branch lines are seen, having actual towns upon them, which nevertheless cannot

pay any dividend at all, and are frequently obliged to appeal for refuge to the "Court." Yet here we have an actual proof of the capabilities of iron and steam to serve a district, and not to forget its shareholders; for, even after allowing something for the cost of land and administration, such as might be contingent on an enterprise carried out without the aid of one great proprietor, there is in the Wotton revenue a good balance on the right side.

It is only right, in concluding these remarks on this curious branch, to say that there are no platforms, the rails weigh only 30 lbs. to the yard, and the line is not fenced except in grazing meadows; at each main road crossing there is a siding for trucks; the guard issues tickets whilst travelling in the train, the tickets being torn from a book as in a tramway-carriage; one ordinary train is instanced as a fair average down, it consisted of the engine, a Great Western railway-carriage, five empty coal trucks, and three trucks laden with hay, which altogether weighed about 50 tons.

The staff of servants working the train consists of one engine-driver, one breaksman, and one guard; at Messrs. Chaplin and Horne's offices, at Brill, there is a manager and two clerks. The principal traffic is in coal, of which from 100 tons to 140 tons go up weekly.

How many parts of England, and more in Scotland and Ireland, are languishing for want of such humble but efficient steam tramways; how enormously might the productive powers of the soil be increased by such easy access to and from the railway system; every farmer might have the railway wagons brought to his homestead, giving him cheap lime, coal, and manure, and taking out his hay, straw, and cattle; and furthermore, what a field is here opened out for the investment of capital now seeking employment and only finding it in foreign enterprises. By a little careful selection of the country, by the co-operation of the landowners, and with the aid of an occasional paper mill, quarry, or manufactory, such undertakings might be made to pay large and handsome dividends, very much exceeding those obtained in the Wotton tramway. Their development and their success must depend on the landowners themselves: if they will obstinately persist in making all kinds of monstrous claims for severance and land, no investor could reasonably be asked to embark in the scheme; but if they would content themselves with fair rent charges and agricultural values, their properties might be benefited in a way to yield them handsome returns.

As such tramways must necessarily follow the surface of the ground to a great extent, avoiding heavy earth-works, it is worth while to consider what really are the limits of gradient.

If we take two pieces of clean iron and lay one on the other, and gradually lift one end of the lower one till the superincumbent piece begins to slide, we shall find that this sliding takes place at a slope somewhere between 1 in 4 and 1 in 6; this, therefore, is the ultimate co-efficient of friction, and varies according to the condition of the surfaces in contact of the metal.

Let the uppermost piece of iron be taken to represent the engine, the lower one the rail, the wheels of this engine being locked and prevented from turning, it will just stand at 1 in 4 to 1 in 6; therefore, if the wheels are caused to revolve, it can just climb this gradient under the most favourable circumstances.

But rain, fogs, and sleet prevent this result from being arrived at in practice, and engineers seem to agree that 1 in 10 is the most that can be climbed in all weathers with certainty; therefore, taking this as the *datum*, up half that gradient, or 1 in 20, the engine can take a load behind it equal to its own weight, and up 1 in 30 twice that weight.

Therefore a 10-ton engine can haul 20 tons up 1 in 30, or two loaded wagons, of say 5 tons each, carrying 10 tons of paying load: the non-paying load being 10 tons.

It would appear, therefore, that 1 in 30 is about the steepest incline which should be adopted for any length; this gives a rise of 176 feet in a mile, and practically commands most countries.

Near Aberdare Junction, on the Taff Vale Railway, ordinary locomotives can be seen regularly working up 1 in 18,—which is a practical proof of the foregoing statements,—they take loads behind them of 45 tons.

Between Manchester and Oldham, where the traffic is enormous, the gradients reach as high as 1 in 27—and ordinary locomotives with coupled wheels climb this, with loads behind them of 60 to 80 tons regularly.

These instances merely show extreme cases neither to be copied nor commended, but where occasion requires to be employed sparingly as precedents.

If 1 in 30 be the worst place on the tramroad, a 10-ton engine could nevertheless haul two cars containing over 150 passengers up it; this would be more than would be requisite in an agricultural district.

Having roughly defined the limit of gradient, let us

finally consider the curves possible, for after all these are the most important questions of all; we have seen that we can get over hills, but we now require to go round corners, or very sharp elbows, as explained before in this paper; the axles of railway carriages are firmly fastened underneath, so that the vehicle has no tendency to follow the curve or lock as an ordinary four-wheel vehicle upon a common road has; hence engineers seldom adopt curves sharper than 660 feet radius on railways, although there are instances as low as 300 feet: the travelling becomes very bad, and the grinding is fearful. That something better can be done has been demonstrated during the last two years; any one can see, in daily use, at the Fenchurch-street station two railway-carriages of four wheels each, mounted on bogies in such a way as to be able to go round very sharp corners; these vehicles are mounted on Grover's patent under-frames, and the results obtained by them in the duration of their tyres, and consequent absence from friction and grind, have been remarkable.

It is true that the ordinary 8-wheeled double bogie vehicle in use on the American railways will do the same thing, but the enormous length and weight of such cars prevents them from being employed profitably on steam tramways; what is really wanted is a short handy vehicle, capable of being shunted and moved about at the station by a couple of men easily.

With respect to the question of engine there is not so much difficulty; a small traction-engine has its wheels very close together, consequently it will take a sharp curve without difficulty; besides which, Mr. Fairlie has constructed engines on his double bogie system, which have immense power, and are capable of going round curves of 50 feet radius; in mountain districts these engines are most valuable, and enable gradients to be worked easily which would otherwise be almost impracticable.

In South America they are in daily use, taking loads of 120 tons up a gradient of 1 in 25, continuous for 11 miles on the Iquique Line, and also on the Mexican railway.

It appears, therefore, that the necessary mechanical difficulties have been practically surmounted; all is ready to hand, the engineer has it in his power to overcome the obstructions which nature has laid in his way, and those only remain which are due to the prejudice of education and human nature; a great lesson has been learnt,—which must be unlearnt,—but the task is not a difficult one if it be met with the spirit of sincere attention and honest endeavour.

A few words more before concluding on the management of existing railways as they stand. It has been taken for granted that, where high speeds are adopted, no substantial change can take place in the strength of the vehicles or the weights of the engines; but why should not express stock be kept distinct, and a considerable reduction be made in that which is meant for ordinary service? Such an economy is being effected on the South London Railway by the present locomotive superintendent, Mr. Stroudley, who has constructed some light and neat carriages, with central buffers, drawn by small engines weighing only 23 tons; these trains are worthy imitation—they are a step in the right direction, and if more fully adopted would give better dividends and reduced fares. The real fact is, that great obstacles are placed in the way of railway officials, in consequence of the division of their responsibility. The locomotive superintendent thinks little about the permanent way—which is not under him, but adopts great strengths and weights, whereby he increases the "life" of his stock. The engineer who has charge of the permanent way complains, but has no remedy. No real improvement is possible until some ruling mind governs each system, and insists upon comprehensive reforms and the adoption of those inventions which guarantee sure economies.

III. CORAL REEFS AND THE GLACIAL PERIOD.

By J. Clifton Ward, F.G.S.
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THERE is nothing which so much helps forward geological science as the study of our globe as it now is. Every fresh discovery in physical geography helps to explain some hitherto mysterious geological fact. The greater part of the world has yet to be travelled over scientifically, and when this is done, the geological science of that day will probably be as much in advance of our present knowledge as our to-day's science is of that of twenty years back.

Geology made a great advance when Darwin explained the mystery of coral reefs, for by their accurate study, geologists learnt how slowly and gradually large tracts of land were submerged, and in what way great thicknesses of limestone could be formed—not by the preservation so

much of actual reefs, as by their disintegration and the widespread deposition of coralline sediment.

Geology made yet another great advance when Agassiz, bringing his knowledge of existing glaciers to bear upon certain phenomena in Scotland and England, showed how certainly our now temperate climate was once an arctic one, and that the diluvial phenomena were for the most part easily explained, on the supposition of the existence of a former glacial period.

Of late, the possibility of determining the time that has passed away since that period of extreme cold has animated the hopes of geologists, and the physicist and astronomer combined have brought their knowledge to bear upon the question. The result, so far, is known to all through the various papers of Mr. Croll. But since truth will stand all shocks, and show itself more truth-like after each attack, it should be the aim of geologists to test all theories put forward to explain series of facts in every possible way; and more especially when those theories are supported by mathematical arguments and reasoning is it incumbent upon the students of nature to see well to the ground-work upon which the mathematician builds his indisputable structure.

It is well known that, according to Mr. Croll's explanation of the cause of the glacial period, those agents producing an extremely cold climate over the greater part of the northern hemisphere would give rise to a proportionally hot one in the southern.

Scientific observation in the southern hemisphere is now bringing before us the fact that, at no very distant period, an extreme glacial climate prevailed there also, and, moreover, that the relics of a former great ice-sheet in the southern hemisphere seem as fresh and of equal value to those of the northern ice-sheet.

Coral reefs are, at the present day, confined within the isotherms of 68°. In proportion as the ice-sheets in either hemisphere are extended into lower latitudes, so must the isotherms of 68° approach the equator, and the coral reef zone become restricted. Let us see what would be the probable result upon the distribution of coral reefs, 1st, of an extended ice-sheet in the northern hemisphere, and, at the same time, an increase of heat in the southern; 2nd, of an extended ice-sheet in the southern hemisphere, and a proportional increase of heat in the northern; 3rd, of greatly extended ice-sheet in both hemispheres at the same period.

1. The study of North American geology shows that, during the glacial period, an ice-sheet completely enveloped

that continent down to the parallel of 39°, while the greater part of Northern Europe was similarly ice-clad.

The present southern limit of perpetually frozen ground in the northern hemisphere is, for a great part of its course, between the parallels of 55° and 60°, though, from the southern point of Greenland to the eastern part of Russia, it runs up to a latitude of 70°. The northern isotherm of 68° roughly corresponds to a latitude of from 30° to 35°, so that now there is an average of 25° of latitude between the southern limit of frozen ground and the northern limit of reef-builders, though, at two points, they approach one another within about 15°. If the former be extended southwards as far as the parallel of 40°, are we justified in concluding that the latter would be thrust southwards in a proportional degree, that is, to a parallel of from 5° to 10°? If so, it is clear that the Equator of Heat, instead of being, for the greater part of its course, north of the equator, as now, might be considerably south of it, even supposing the climate of the southern hemisphere to be no warmer than at present. But if, as we are supposing, the southern hemisphere was under a very hot climate, the equator of heat might be still farther removed from the geographical equator.

Under this condition of things, it would seem certain that there could be but a small range of reefs north of the equator, but that they might extend farther south than at present. Supposing the climate of both hemispheres slowly to approximate to that which now prevails, it is evident that the oldest reefs would be found south of the equator. Now the atolls undoubtedly furnish the evidence of greatest antiquity, since their formation—on Darwin's view of their origin—clearly shows a very gradual sinking of land throughout immense periods of time. Hence we should expect to find the greatest number of atolls south of the equator, and the reefs north of it to belong mostly to the classes of fringing and barrier reefs, which is indeed found to be the case. Moreover, could we hit upon some sure average rate of the growth of reefs, and know the exact relation which such rate of growth bears to the rate of subsidence, or otherwise, of the land, we should have, in these more northern reefs, some indication of the time that has elapsed since the close of the glacial period in the northern hemisphere.

In an article in the "Geological Magazine" for January, 1869, I suggested that the present distribution of coral reefs seemed to show that those south of the equator were of much greater age than those north of it, that the sinking of the supposed old Pacific continent, perhaps, commenced

long ere the beginning of the glacial period in the northern hemisphere, and continued uninterruptedly all through that period, the atolls being slowly built up throughout the whole of that time, and that, as the northern climate became finally milder, they were gradually extended further north. Dana estimates the subsidence in the Pacific area as not less than 6000 feet, and taking the rate of subsidence and the upward growth of a reef as 1 ft. per century, this would give a period of 600,000 years for the formation of the Pacific atolls, without allowing for any time of intermittent upward movement, or times during which there might be little or no movement in either direction. The instance of the Florida reefs was also brought forward; here some 10 reefs, one within the other, have been formed, probably since the ice-sheet disappeared, only 9° farther north, and each reef being taken at 70 ft. in thickness, and the rate of growth as above, a period of 70,000 years is given for their formation. Hence, on the whole, the present distribution of coral reefs, especially of atolls—for the most part south of the equator—would seem to favour the idea of a glacial climate having prevailed in the northern hemisphere at a much more recent period than in the southern. But, on the other hand, it may be argued, that atolls, perhaps, do not occur north of the equator in any abundance, because the requisite sinking land was not present, and this argument may hold good to a certain extent, especially as it is the very existence of atoll reefs that marks in great measure the broad land of subsidence in the Pacific.

2. Let us now take the case of an extreme glacial climate in the southern hemisphere, and a proportional increase of heat in the northern.

Agassiz, in the results of his South American Expedition, has just shown that an ice-sheet probably enveloped the southern part of South America, down to the latitude of at least 37°, and even supposing the sheet not to have extended so far as that in the north did, on account of the less amount of continental land round the southern pole, are we not justified in concluding that the southern isotherm of 68° and the equator of heat itself would be shifted considerably north, and the growth of coral reefs rendered as *generally* impossible south of the equator as they probably were north of it during the undoubted glacial period? Supposing no glacial period to have visited the northern hemisphere from the time of extreme southern glaciation until now, we should have expected to find north of the equator coral reefs of great thickness, and atolls in great abundance, *provided*

only the requisite slow subsidence occurred in the northern equatorial region. And the absence of a great coral reef development, in the shape of atolls, north of the equator, points, therefore, either to the want of the requisite slowly subsiding area, *or* to the advent of a cold period *subsequent* to that occurring over the southern hemisphere, and, therefore, checking the coral growth.

3. What now would probably be the condition of things, supposing the extreme glacial climate occurred simultaneously in both hemispheres? Agassiz says,* "Let me state that I have not noticed anything to confirm the idea that the glaciers of the northern hemisphere have alternated with those of the southern hemisphere in their greatest extension, as is assumed by those who connect with the precession of the equinoxes the difference of temperature required for the change. The abrasions of the rocks seemed to me neither more nor less fresh in one hemisphere than in the other. . . ."

Undoubedly, the extreme glaciation in both hemispheres is the most recent of geological changes; both north and south of the equator it is of younger date than the late Tertiary deposits. Since, however, a Miocene, or Pliocene, fauna and flora may not be of the same age precisely in both hemispheres, time being required for the slow progress of new animals and plants into far latitudes, it follows that the glaciation, though affecting rocks of these ages, and therefore posterior to them, may not be of equal age both in the north and south. Granted, however, that the period of glaciation was approximately the same in both hemispheres, does it not follow that tropical life would be hard put to for a place of abode?

In this case the two isotherms of 68° would be made to approach each other from both hemispheres, and the equatorial belt inhabitable by reef-builders very much narrowed; in fact, it is conceivable that during such a simultaneous maximum of cold in both hemispheres the combined effect on equatorial heat would be such as to squeeze out, as it were, some forms of tropical life, and confine others to very narrow bounds. On the present supposition, therefore, if the climate ameliorated slowly from the time of extreme cold north and south, to the present day, we might expect to find the thickest reefs or the greatest number of atolls close about the equator, always provided that there were areas of subsidence sufficient to allow of the free growth of atolls.

* See his Report of South American Expedition, as given in "Nature," Aug., 1872, p. 272.

There is another point which might perhaps throw some light upon the question. It is well known that high equatorial lands or corn lands south of the equator are tenanted by north temperate forms of life, left on the mountain ranges in such latitudes as the extreme cold of the glacial period decreased. Darwin says,* "From the presence of temperate forms on the highlands across the whole of equatorial Africa, and along the peninsula of India to Ceylon and the Malay Archipelago, and in a less well-marked manner across the wide expanse of tropical South America, it appears almost certain that at some former period, no doubt during the most severe part of the glacial period, the lowlands of these great continents were everywhere tenanted under the equator by a considerable number of temperate forms. At this period the equatorial climate at the level of the sea was probably about the same with that now experienced at the height of from 5000 to 6000 feet under the same latitudes, or perhaps even rather cooler. During this, the coldest period, the lowlands under the equator must have been clothed with a mingled tropical and temperate vegetation."

Now if both hemispheres were simultaneously visited by an extreme glacial period, we should expect to find about an equal share of northern and southern forms left about the equatorial highlands, unless, indeed, the ice-sheet was much more developed in the one hemisphere than the other, which might arise from larger areas of land on one side of the equator than the other. At all events we should not expect to find a very marked preponderance of forms from one side, especially along those equatorial parts with continental tracts of land both north and south.

Again, if the extreme glacial climate has visited the southern hemisphere at a later date than the northern, we should expect to find a preponderance of south-temperate forms of life on equatorial highlands rather than of north-temperate forms, especially along those parts of the equatorial belt where there was continental land to the south. Lastly, if the glacial period prevailed in the northern hemisphere some time after the last cold era in the southern, the north-temperate forms of life would prevail on equatorial highlands almost to the exclusion of south-temperate—these latter being the relics of a former southern cold. What do we actually find to be the case as regards this distribution of north- and south-temperate forms respectively? I again

* Origin of Species, p. 455.

quote Darwin*—"It is a remarkable fact, strongly insisted on by Hooker in regard to America, and by Alph. de Candolle in regard to Australia, that many more identical or now slightly modified species have migrated from the north to the south than in a reverse direction. We see, however, a few southern forms in the mountains of Borneo and Abyssinia. I suspect that this preponderant migration from the north to the south is due to the greater extent of land in the north, and to the northern forms having existed in their own homes in greater numbers, and having consequently been advanced through natural selection and competition to a higher stage of perfection or dominating power than the southern forms."

The fact is evident, then, that northern forms more encroach upon equatorial and southern regions than southern upon equatorial and northern. Is this due wholly to the greater extent of northern land, as Darwin suggests, or to the southward driving force of a cold period having acted *more recently* than the northward driving force; in other words, does it not point to a glacial period having prevailed in the north more recently than in the south?

From this brief consideration of the probable effects of—(1) latest glaciation in northern hemisphere; (2) latest glaciation in southern hemisphere; (3) simultaneous glaciation in both hemispheres; what are our results? Mainly these, I think:—

1. That an extended ice-sheet in the northern hemisphere would necessarily thrust farther south the equator of heat, and consequently the two isotherms of 68° within which limit the reef-builders occur.

2. That this effect would be increased by the southern hemisphere being unduly hot, just in proportion as the northern was unduly cold.

3. Under these conditions coral reefs would occur in greatest force south of the equator, and, supposing the climate in both hemispheres slowly to approximate to what it is at present, we might now expect to find the oldest—and, therefore, probably the thickest—reefs south of the equator.

4. Could some sure standard of the growth of coral-reefs be established, we might, by comparing the reefs north and south of the equator, form some idea of the shortest period of time which could have elapsed since an extreme glacial climate prevailed in either the one or the other hemisphere.

* Origin of Species, 5th edition, chap. xi., p. 457.

Example: The reefs of Florida may have taken 70,000 years in formation—therefore the glacial climate, with its ice-sheet extending south into 39° north latitude, cannot have ceased *less than* 70,000 years ago, because these reefs have every appearance of having been uninterruptedly formed, and not of being partly pre-glacial and partly post-glacial.* Again, the South Pacific atolls represent a sinking of 6000 feet; this at 1 foot per century, without allowing for intervals, would give 600,000 years for their formation, which from the very nature of atolls must have been continuous—therefore a glacial climate, with an ice-sheet on the continental lands extending north into 37° south latitude, could not probably have existed in the southern hemisphere within that period.

5. It must, however, be remembered that one of the conditions for the formation of atolls is the existence of an area of slow subsidence, which condition may not have occurred in the northern hemisphere within the 68° isotherm.

6. If an extreme glacial climate occurred *latest* in the southern hemisphere, and coincidently with it an extreme hot climate in the north, we might expect to find the oldest coal-reefs *north* of the equator.

7. An extreme glacial climate prevailing in both hemispheres simultaneously would restrict the coral reefs to very narrow limits on either side of the equator, unless, owing to a less amount of land in the southern hemisphere, the ice-sheet there should not be so continuous and extensive, in which case irregularities in their distribution might occur. On this supposition, the age of the most southerly South Pacific atolls would probably indicate the least time that could have elapsed since the ice-sheet disappeared, since atolls necessitate a continuous act of formation, and they could not be formed partly in pre-glacial and partly in post-glacial times.

8. If the glacial climate prevailed last in the northern hemisphere, we should expect to find north-temperate forms of life more numerous than south-temperate on equatorial highlands; if the southern hemisphere was the most recently glaciated, the south-temperate forms would be more abundant on the equatorial highlands than the north-temperate; if both hemispheres were glaciated at the same time there would be about an equal mingling of north- and south-temperate forms about the equator.

9. The facts of the case are—(1) That the largest

* I believe I am right in saying that there is no evidence of a long break in their formation.

number of atolls and the thickest reefs occur south of the equator; (2) That the north-temperate forms of life in equatorial regions greatly exceed the south-temperate forms.

10. The general conclusion from these facts seems to be, that the northern hemisphere suffered glaciation at the latest period, and that the southern hemisphere was glaciated at a time perhaps more remote from the period of the northern glaciation than that is from the present.

IV. THE PLANET MARS IN 1873.

By Richard A. Proctor, B.A. (Cambridge),
Honorary Secretary of the Royal Astronomical Society.

THE planet Mars, after being unfavourably placed for observation during two years, has returned to a position where he can be studied advantageously. On April 27th he will be in opposition, and therefore due south, or at his highest above the horizon at midnight. Then, also, he will present his largest apparent disc, or very nearly so (at least so far as the present opposition is concerned). Moreover, as will be seen farther on, there are circumstances which render the study of the planet while in our neighbourhood particularly interesting on this occasion. Therefore the opportunity seems a favourable one for entering into the consideration of the various facts of interest which have been made known respecting Mars. But inasmuch as I have on more than one occasion discussed the principal features of the planet, I shall here restrict myself as far as possible to circumstances presenting some degree of novelty, and devote much of my space to the suggestion of observations which should be made by telescopists during the next two or three months.

It will be known to most of my readers that the planet Mars is the only primary member of the solar system whose condition can be studied under circumstances sufficiently favourable to enable us to arrive at satisfactory conclusions respecting the planet's physical condition. Venus approaches us more nearly, and is more brilliantly illuminated; but when Venus is at her nearest she lies directly towards the sun, and her unillumined side is turned towards us. She is more favourably seen when near her elongations, but is then much farther away than Mars at his nearest, at which time he (unlike Venus) is most favourably situated for observation. Moreover, the great brightness with which Venus

is illuminated renders the study of her surface exceedingly difficult. A similar remark applies to Mercury; and it need hardly be said that the proximity of Mercury to the sun presents a yet more serious difficulty to the telescopist, in the fact that Mercury is never far removed from the sun as respects apparent position.

On the other hand, when we pass from Mars to the other superior planets, we find that we must necessarily study the surface even of Jupiter and Saturn under conditions very much less favourable than those which exist in the case of Mars. When at his nearest, that is, when he is in opposition near the perihelion of his orbit, Mars is but about 35 million miles from the earth; and even when he is in opposition near aphelion his distance does not exceed 61 million miles. But Jupiter is never less than 360 millions of miles from the earth, and Saturn never less than 732 millions of miles. So that taking the case of Jupiter at his nearest as compared with Mars in opposition near perihelion, we see that in the first place Jupiter is more than ten times as far away, and the apparent dimensions of equal parts of his surface are therefore reduced more than a hundred times as much, and also Jupiter is very much less brilliantly illuminated by the sun. For the least distance of Jupiter from the sun is 452,692,000 miles, the least distance of Mars 126,318,000 miles, the former distance exceeding the latter about $3\frac{3}{5}$ times; and as illumination varies as the square of the distance, it follows that equal surfaces of Jupiter and Mars (both in perihelion) receive from the sun quantities of light in the proportion of about 13 to 1. Now this consideration is important in comparing the circumstances under which we study Jupiter and Mars. For, although in the case of Venus we have spoken of a degree of brightness which interferes prejudicially with observation, yet in the case of Mars and Jupiter we are not troubled with an excess of light, insomuch that the smaller quantity received from (equal surfaces of) Jupiter introduces a difficulty. When the highest magnifying powers are used, on the best observing nights, there is a want of luminosity in the disc of Jupiter which renders the study of his surface more difficult than it would otherwise be.*

* The intrinsic brightness of Jupiter is not reduced to the same extent as the quantity of light received by equal portions of his surface (compared with that of Mars). Whether this be owing to the greater reflective power of his surface (that is, of the surface which forms his visible disc), or to some inherent luminosity possessed by the planet, is not as yet determined. *Adhuc sub judice lis est.* But that the peculiarity is very noteworthy will appear from the considerations discussed farther on. In fact, Zöllner estimates the reflective power of the surface of Jupiter at more than twice that of the surface of Mars, or greater in the ratio of 624 to 267. Prof. Bond (the elder), of America, estimated the reflective power of the surface of Jupiter still higher.

It is hardly necessary to inform the reader that the point of chief interest in the study of Mars is the determination of the degree in which he resembles or differs from our earth. And in one respect the discussion of this question is more interesting than it would be in the case of a planet like Venus, which is the equal (or nearly so) of the earth in size. Mars is so much smaller than the earth, that although he belongs to the same family—that is to the inner or so-called terrestrial family of planets—the question might well arise whether he belongs in reality to the same order. He is, in fact, nearer to our moon in volume than to the earth, and comes about midway between the earth and moon as respects mass.* Now we know that the moon is totally unlike the earth in all the circumstances which we associate with the requirements of living creatures; and therefore it might well be believed that Mars is as likely to resemble the moon as the earth in this respect, and is even more likely to occupy a position in the scale of creation utterly unlike that which is occupied by either the moon or the earth. It is the discussion of this question, in the light of the evidence obtained by observation, which renders the study of the planet Mars so full of interest.

I will briefly recapitulate what is known about Mars, noting that the matter is more fully dealt with in my "Other Worlds," and in a paper on Mars in my "Essays on Astronomy." It is necessary for me to allude to these prior discussions of my subject, since otherwise the present paper might seem wanting in completeness. My purpose is to treat as briefly as possible of those matters on which I have already touched, in order that as much as possible of the present essay may deal with new matter.

We know first that the surface of Mars is divided into land and water; and that the continents and oceans of Mars have the shapes depicted in the projections which (for another primary purpose, however) illustrate the present essay. The land has a tint suggesting the idea that the chief constituent of the soil may be a substance resembling our sandstones; though on this point it would be unsafe to

* By this I do not mean that his mass is nearly the arithmetical mean between the mass of the earth and that of the moon, but nearly the geometrical mean. Thus the mass of the earth being taken as unity, that of the moon is 0·0114, and the arithmetical mean of these quantities is 0·5057. Now the mass of Mars is 0·118, or less than a quarter of this arithmetical mean. But the geometrical mean between 1 and 0·0114 is about 0·107, which approaches nearly enough to the value of the mass of Mars to justify the remark in the text. It must not be forgotten, however, that as respects the actual quantity of matter he contains, Mars resembles the moon more nearly than the earth.

speculate too confidently, since the observed colour is probably produced by the blending together of a great number of different tints. That the bluish tracts on Mars are oceanic may be inferred almost with absolute certainty, simply because we know that the atmosphere of Mars is at times loaded with considerable quantities of the vapour of water. This the spectroscope has told us; for it need hardly be remarked that for the lines due to water vapour to be seen at all in the spectrum of the planet, the quantity of aqueous vapour then present in the Martial atmosphere must be very great. I remark in passing, to remove possible misapprehensions on the part of those who are unfamiliar with Dr. Huggins's researches on Mars, that he has shown beyond dispute that the water-lines in the spectrum were not due (at the time of observation) to our own atmosphere. Then this result agrees excellently with the observations which had been made for many years on the white spots near the poles of Mars. Sir W. Herschel had come to the conclusion that these spots are the polar snows of Mars, partly because this conclusion seemed justified by terrestrial analysis, and partly because the white spots waxed and waned in magnitude, in accordance with the theory that they are snowy regions waxing in winter and waning in summer. Snows cannot be produced without large water-covered regions; and the bluish tracts have precisely the appearance which we should expect Martial oceans to present. Then, we have already seen that the spectroscope gives evidence of aqueous vapour in the Martial atmosphere. There is then, therefore, a permanent atmosphere (for no physicist can entertain for an instant the belief that the Martial atmosphere consists solely of aqueous vapour). There must, moreover, be winds, and probably clouds and rain, besides those other meteorological phenomena depending on the increase and diminution of the quantity of aqueous vapour present in the atmosphere. Accordingly, the telescopist finds ample evidence of such phenomena in the appearance of Mars. He finds that known Martial lands and seas are often concealed from view, as if under a layer of clouds; he has been even able to watch the gradual dissipation of such cloud-layers, as if under the rays of the sun as it rose higher in the Martial skies. He sees all round the disc of Mars a whitish light, which can be explained as due to rounded or cumulus clouds in the Martial atmosphere.* He notes the greater distinctness of

* In my "Essays on Astronomy," I show that this explanation is available; and I add as another explanation, the possibility that the morning and evening

the hemisphere of Mars, which at the time of observation is passing through its summer season, and readily interprets the indistinctness of the other hemisphere as due to greater prevalence of clouds during the Martial winter. He can even recognise a difference in the colour of the planet as a whole, as though at certain times there were a great increase or diminution of the total quantity of cloud in the Martial air.

All these circumstances indicate a resemblance rather to our earth than to the moon, where, as we know, there is neither water nor any considerable atmosphere; and when we consider the physical relations involved by the circumstances thus far noted, we find much to suggest the idea that Mars deserves to be regarded as a miniature of our earth. It seems reasonable to infer that since the regions where snow is constantly present, extend on Mars to latitudes resembling those which limit our own regions of perpetual snow, there must be a certain climatic resemblance between the two planets, notwithstanding the fact that Mars receives so much less heat (on mile per mile of surface) than the earth. If we remember that the mean distance of Mars from the sun exceeds that of the earth in the proportion of more than 15 to 10, so that the supply of heat from the sun is less in the proportion of 100 to 225, we cannot but be surprised to find that any resemblance of the sort should exist. And yet, unless we adopt a view presently to be discussed, I apprehend that very little doubt can exist upon the subject. For although, as has been well pointed out by Prof. Tyndall, the presence of snow is an indication of the action of heat, it is manifest that it must indicate also the existence of cold, and that the relative extent of the permanent snow regions of a planet must form

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skies of Mars are ordinarily clouded. But although the second explanation is obviously in accordance with the whiteness near the edge of the disc (since at the parts near the eastern edge day is breaking on Mars, while at the parts near the western edge evening is approaching), the explanation must, nevertheless, be abandoned in presence of the fact that near the terminator of gibbous Mars there is a marked loss of brightness. For here if there were a misty sky on Mars, the whitish light should be seen, and would compensate for the greater obliquity of the sun's rays. As such light is not seen near the terminator, the influence clearly is, that the morning and evening skies of Mars are not specially cloudy, but that the white light seen near the edge of the disc depends (according to the first explanation) on the obliquity with which the line of sight falls on those parts. The illustration in my "Essays" shows how this obliquity would result in causing the whole of the light here received to be that reflected from clouds. I do not think any other explanation is possible; certainly I cannot conceive that any reliance can be placed on the influence of Zöllner, that Mars is covered over with hills having a mean slope of 72°. The great point to be determined, however, is whether the edge of the terminator does or does not show signs of evening or morning mists.

a reliable indication of the general climate of the planet. Indeed, it must be noted that when he pointed out the fact to which I have referred, Prof. Tyndall offered no explanation of it. He simply noted the error of those who would seek to explain the former presence of enormous glaciers solely by the action of cold. "Vast masses of mountain ice indicate infallibly," he said, "the existence of commensurate masses of atmospheric vapour, and a proportionately vast action on the part of the sun. In a distilling apparatus, if you require to augment the quantity distilled, you would not surely attempt to obtain the low temperature necessary to condensation by taking the fire from under your boiler; but this, if I understand them aright, is what has been done by those philosophers who have sought to produce the ancient glaciers by diminishing the sun's heat. It is quite manifest that the thing most needed to improve the glaciers is an *improved condenser;* we cannot afford to lose an iota of solar action; we need, if anything, more vapour, but we need a condenser so powerful that this vapour, instead of falling in liquid showers to the earth, shall be so far reduced in temperature as to descend in snow. The problem, I think, is thus narrowed to the precise issue on which its solution depends." Now, it is important to notice that what is here affirmed of glaciers does not apply with equal force to snow regions at the poles of a planet like the earth or Mars. All the snow which covers these regions must have been formed originally by the action of heat. But a degree of heat, very moderate in amount, would cause the evaporation of sufficient quantities of vapour to produce the snow which covers a widely-extended region. In fact, we know that even in the arctic regions mists and clouds are formed, whence eventually snow is produced, and that these mists and clouds are *not* due in all cases to aqueous vapour which has been formed in warmer latitudes, but are actually produced over ice-covered regions in calm weather; when, therefore, no air is arriving from warmer places. Now, manifestly the snow which covers the polar regions of Mars must either have been formed from vapour raised and condensed in those very regions, or else from vapour raised in lower latitudes and condensed near the poles. In the former case, there must be heat enough in the arctic regions to produce evaporation, and therefore, *à fortiori*, the heat in lower latitudes must in that respect resemble the heat we experience in our temperate zones. In the other case, there must be great processes of evaporation, corresponding to those which take place on our earth; there must be winds carrying the moist

air polewards (whence, necessarily, winds blowing towards the equator may be inferred); and there must, in fact, not only be general climatic relations resembling those on our earth, but also similar meteorological phenomena.

It is not so easy as has been sometimes supposed (by myself amongst others) to decide between these two solutions. All that the telescope reveals in Mars has been held to show that the latter solution must be accepted. We actually appear to see the clouds, which are formed in Martial temperate regions, showing that great quantities of aqueous vapour are commonly present in the atmosphere over these regions. We know that more heat than that which would evaporate aqueous vapour near the arctic regions must necessarily be expended on the great oceans of Mars, and that therefore aqueous vapour must be raised into the air over these oceans. And we have seen that spectroscopic analysis confirms this conclusion, or rather establishes it as a demonstrated fact.

But we are thus brought into the presence of somewhat serious difficulties.

In the first place, let us remember that the direct supply of heat from the sun is certainly that which has been mentioned above. In other words, the surface of Mars receives, mile for mile, less than 4-9ths of the heat which our earth receives. This heat may be treasured up (as it were) more completely, or owing to some cause unknown may act more efficiently; but there can be no question that no greater amount of heat is actually received. So that we have this first difficulty to encounter, that regarding Mars as a whole, he seems to be more than twice as well warmed as in the nature of things he would be, supposing the condition of his surface and of his atmosphere resembled what we are acquainted with on earth.

But now as to his atmosphere. Let us suppose that it is constituted like the earth's atmosphere, and let us enquire what must be its density and pressure under such and such conditions. But first it may be asked whether we may not be justified in forming some such opinion as to the quantity of air around Mars which is indicated in Mr. Williams's work "The Fuel of the Sun." Here, as is probably known to many of my readers, the assumption is made that every celestial body has a certain proportion of air around it, a proportion somewhat artificially determined by Mr. Williams, as depending on a numerical relation, the necessity of which is not demonstrated by the evidence. Nevertheless, it seems a reasonable assumption that the larger bodies

should have a vaporous envelope of greater extent, whether we regard such envelope as originally a portion of the once wholly vaporous mass of the planet or as partially gathered in by the planet's attraction on vaporous matter in the inter-planetary spaces. And if we assume that the quantity of atmosphere would be proportional to the mass of the planet,—that is to the centre or third power of the planet's radius, multiplied by the number representing the density of the planet,—then since the surface of the planet is proportional to the square of the radius, it would follow that the quantity of air vertically above each square mile of a planet's surface would vary directly as the product of the numbers representing the diameter and the density of the planet. This will be thought as probable a conclusion as Mr. Williams's, and in the present instance it leads to a very similar result. We may adopt it provisionally, in order to see what general results we obtain by following such considerations.

Applying this rule to Mars, whose diameter is about 6-11ths, and density about 3-10ths of the earth's, we obtain for the quantity of air above each square mile of the surface of Mars, the expression $\frac{6}{11} \times \frac{7}{10}$, or $\frac{21}{55}$, where the corresponding expression of the earth is unity,—so that, quite nearly enough for our present purpose, the quantity of air above each square mile of the surface of Mars would be 2-5ths of the quantity above each square mile of the earth's surface. But the pressure and therefore the density of the air at the mean level of a planet depend on the quantity of air above each unit of area, and the attraction of gravity at the planet's surface; for this pressure is solely produced by the weight of the air. Gravity on Mars is represented by 0·387, where terrestrial gravity is unity; and multiplying $\frac{2}{5}$ by 0·387 we obtain 0·1548, which represents (on our assumptions) the pressure of the atmosphere on Mars, when unity represents the atmospheric pressure at our sea-level. Mr. Williams deduces from his assumption a pressure of 0·179. According to one view, the mercurial barometer would stand at about $4\frac{3}{4}$ inches; according to the other, at about $5\frac{1}{2}$ inches on Mars.

Now it is not difficult to perceive that with an atmosphere such as this, and a supply of solar heat equal only to 4-9ths of that which we receive from the sun, Mars might present most of the appearances actually observed. This has been shown (very ably, in my opinion) by Mr. Williams; and although I shall proceed presently to

consider certain features suggesting a different theory, I must point out that the balance of evidence appears to me to be decidedly in favour of his theory. Meantime I will follow the line of reasoning pursued by Mr. Williams, noting that much of what he says must be regarded as following obviously from the theory on which it is based.

In the first place, it is clear that with so shallow an atmosphere and so small a direct supply of solar heat, the cold in Mars would be intense. The mean temperature would be below the freezing-point. Nevertheless in the day time, especially in low latitudes, the heating power of the solar rays would be considerable. It would not be so intense as on the summits of our loftier mountains, when a mid-day sun is pouring his rays on the snow-masses there, but would correspond rather to the heat of the sun at about ten or eleven on a summer's morning in Switzerland. It would certainly suffice to melt any surface snows, and also the surface ice of the Martial oceans, which on the theory we are considering must be regarded as frozen throughout their depth.

Now, in considering what would follow as the day proceeded, we find some difficulty in deciding whether there would be an inflow toward the warmed mid-day regions or an air-current flowing outwards (we are speaking now of surface-currents). On earth there is a flow of air towards the region where evaporation is taking place, and it has been urged that this is due to the fact that the aqueous vapour, rising by reason of its relative lightness, causes upward currents in the permanent atmosphere, and that thus an indraught is produced. On the other hand, where evaporation proceeds rapidly, there is a great addition to the atmosphere and consequently an increase of pressure, which would tend to occasion an outflow. In the case we are dealing with, the latter effect might prevail; but in any case it is not perhaps very important to consider the question; because, whether the surface-flow were towards or from the region of evaporation, there would be a flow of moisture-laden air from that region. In one case it would be a surface-flow, in the other it would be an upper-air current; but it is immaterial, so far as our present purpose is concerned, whether the outward flow took place in the upper or lower regions of the air.

Then as the day proceeded, and some considerable time before sunset, "a feathery hoar-frost" would begin to fall. "There would," in fact, "be the same kind of action which Sir J. Herschel has described as necessarily taking place in

the moon if any water exists on that satellite, and which he compares to the *cryophorus* experiment. There should, however, be some difference between the case of Mars and the moon. The vacuum of Mars being only comparative, the action would be much slower and less decided than in Sir J. Herschel's supposed case; and the mean temperature of Mars being so much lower, the freezing-point and consequent precipitation of a haze of hoar-frost must commence considerably before reaching the actual boundary between light and darkness; at that angular distance, in fact, from solar verticality, where the cooling influences of the planetary radiation,—aided by those of the remaining ground-ice,—must reduce the surface temperature to the freezing-point." "Thus," proceeds Mr. Williams, "there would be no great well-defined masses of vesicular vapour floating irregularly, like our clouds, in the atmosphere of Mars,—no cumulus, no cumulo-stratus, nor even cirro-cumulus clouds; and, excepting at the borders of the Polar ice, nothing denser than a thin veil of stratus or cirro-stratus cloud, formed of ice-crystals,—the kind of cloud or mist which in our atmosphere makes halo round the moon, and only hides her face sufficiently to exaggerate her beauty, like the gauze 'complexion-veil' of the coquette. The mid-day region, and a certain distance round it, would but rarely be subject to this small degree of obscuration, as the sun's heat there should under ordinary circumstances hold all the vapours it had raised in complete and transparent solution."

It will be gathered, from what has been already stated, that while the results thus indicated accord well with the general features of Mars, they do not agree with the observed appearance of the terminator, when Mars is gibbous. I pause to note this circumstance, because it is manifestly important that observation should be specially directed to the examination of the actual brightness near the terminator of Mars; and it chances that, as will presently be more particularly indicated, the present opposition-period of Mars is particularly well suited for the observation of this feature. But it may be also well to note in this place, that in one circumstance the aspect of Mars corresponds well with Mr. Williams's theory. Mr. Dawes makes the following remarks, in describing the appearances presented by Mars during the opposition of 1865, when the planet was particularly well placed for observation:—"On the whole," he says, "my impression has been that Mars has not usually a very cloudy atmosphere. During the last

opposition, the permanence and nearly equable distinctness of the principal features, under similar circumstances, was surprising. On no occasion could I satisfy myself that any part was decidedly less distinct than might be expected from the appearance of the other features then visible. The very white spots noticed on a few occasions, which certainly gave the impression of masses of snow or the reflection from the upper surface of masses of cloud, formed the only decided exception, unless we include the somewhat remarkable fact that the short and rather thick dark line plainly seen near the North Pole on November 14th was invisible on the 12th, when the narrow strait extending from that part of the northern hemisphere towards the south and other objects in the same vicinity were well seen. On November 10th, also, the northern extremity of that narrow strait was invisible, though it might have been expected to be quite as well seen as on the 12th, and even better than on January 22nd. These exceptions to the prevalent clearness of the Martial atmosphere, both relate to regions in high Martial latitudes, and therefore literally tend to 'prove the rule.'"

We come next to the very natural and effective explanation of the Martial snow-caps, in Mr. Williams's theory. We have seen how, under the supposed circumstances, there would be a deposition of hoar-frost continually taking place all round the disc of Mars. Now, "the rotation of the planet will produce," as Mr. Williams points out, "a considerable difference in the results of this deposition. All that falls on the east and west sides of the planet will be thawed and evaporated by the next day's sunshine,* so that the maximum accumulation in these directions can be but one night's deposition; but on the north and the south there will be continual accumulation, which will only be thawed up to a certain latitude by the annual summer presentation of either hemisphere to the sun." The distance between the mean limits of the north and south patches of accumulated hoar-frost may be taken as an approximate measure of the diameter of the circle over which the sun's rays are capable of raising the day-time temperature above the freezing-point (or rather perhaps, of melting quite through the deposited layer of light snow)." Here Mr. Williams notes a consideration which suggests an interesting point for observation. He remarks that the boundary of the

* The part on the west is actually coming into sunshine, so that "the day's sunshine" would be a more correct expression than "the next day's sunshine," as respects this part of the planet.

region, where the evening deposition of hoar-frost was in progress, should not appear so sharp and well defined as the limit of the morning thaw.

We come next to a rather sensational feature of the theory, or rather of the consequences attributed to it: "At the poles," says Mr. Williams, "and for some distance around them, the annual amount of deposition must exceed the annual amount of thawing and evaporation, and therefore a gigantic glacial mountain must there accumulate, with a continual growth and tendency to assume a conical form. As the deposition of ice-crystals would commence before actual sunset, and would probably reach its maximum or even be finished before reaching the boundary line of day and night, in consequence of the thinness of the atmosphere of Mars and the resulting rapidity of radiation, the building up of this polar mountain would be very irregular. In mid-winter, the lower slopes of its sides would receive the greatest accessions. With the advancing line of daylight the elevation of the zone of maximum deposition would increase until it reached the summit. This coincidence of maximum deposition with the summit would occur twice a year, before and after midsummer. During the summer, the only regions receiving any deposition at all would be the summit and its immediate vicinity; while, at the same time, its sides would be rapidly thawing by the powerful action of the continuous sunshine of the long arctic summer day. At this season, the slopes of the arctic mountain would be riven by gigantic ice-floods and water-floods, avalanches, glaciers, and torrents."

While admitting as almost a necessary consequence of the supposed condition of Mars that there would be an accumulation of snow towards the poles of the planet, I must confess I cannot follow Mr. Williams in assuming that the snow-caps can attain a thickness sufficient to increase perceptibly the apparent diameter of the planet. It is true that the telescopist recognises an apparent projection of the polar snows beyond the circular outline of the disc. But irradiation affords so sufficient and satisfactory an explanation of this circumstance as to leave in my opinion little to be desired; whereas the accumulation of snowy masses to a depth of several miles appears difficult to accept, when it is remembered how relatively small must be the quantity of aqueous vapour which could be raised into the tenuous Martial atmosphere. Nevertheless, as Mr. Williams has advocated with some ingenuity the theory not only that such masses exist, but that great glacial catastrophes

occur which are recognisable by the terrestrial telescopist, I shall venture to quote some observations by the late Gen. Mitchel (the American observer), which seem to accord singularly well with that rather startling theory.

First, let us examine what in Mr. Williams's opinion would happen and be seen :—" The tendency of the summer growth of the summit and undermining of the sides would be," he remarks, " to bring about periodical catastrophes, by the more or less complete toppling over of the mountain cone in the form of a gigantic avalanche. The occurrence of such a catastrophe would be most sensibly indicated to a terrestrial observer by an irregular and temporary extension of the polar whiteness; where the *debris* of the great avalanche had been hurled beyond the general glacial boundary, and had usurped the region of the summer thaw." The evidence quoted by Mr. Williams himself is an observation made by Prof. Phillips, of Oxford, and two practised observers—Messrs. Luff and Blorridge, working with him. " We noticed," says Prof. Phillips, " a gleaming mass of snow very distinct, so much so, that as happened with the south polar snow of 1862, it seemed to project beyond the circular outline, an optical effect no doubt due to the bright irradiation." On this Mr. Williams remarks that, although Prof. Phillips attributes this appearance to irradiation, it may have been due to the actual heaping of the avalanche material of the overthrown polar ice-cone. But the following observations by Mitchel seem far more strikingly to favour Mr. Williams's bold and ingenious hypothesis :—

" I will here record," says Mitchel, at p. 89 of his " Popular Astronomy," " some singular phenomena connected with the 'snow-zone,' which, so far as I know, have not been noticed elsewhere. On the night of July 12, 1845, the bright polar spot presented an appearance never exhibited at any preceding or succeeding observation. In the very centre of the white surface was a *dark spot*, which retained its position during several hours, and was distinctly seen by two friends who passed the night with me in the observatory. It was much darker and better defined than any spot previously or subsequently observed here; and, indeed, after an examination of more than eighty drawings at previous oppositions, I find no notice of a dark spot ever having been seen in the bright snow-zone. On the following evening no trace of a dark spot was to be seen, and it has never after been visible." This is singularly suggestive of the falling away of a great portion of the snow-cone, followed very soon (as would naturally happen) by the snowing over of

the cavity thus formed. The other observation is fully as singular:—"On the evening of August 25, 1845, the snow-zone, which for several weeks had presented a regular outline nearly circular in appearance, was found to be somewhat flattened at the under part, and extended east and west, so as to show a figure like a rectangle with its corners rounded. On the evening of the 30th August, I observed for the first time a small bright spot, nearly or quite round, projecting out of the lower side of the polar spot. In the early part of the evening the small bright spot seemed to be partly buried in the large one. After the lapse of an hour or more my attention was again directed to the planet, when I was astonished to find a manifest change in the position of this small bright spot. It had apparently separated from the large spot, and the edges of the two were now in contact, whereas when first seen they overlapped by an amount quite equal to one-third of the diameter of the small spot. On the following evening I found a recurrence of the same phenomenon" (in other words, the phenomenon was shown to be optical, and depending on the relative positions of two great snow-masses). "In the course of a few days," proceeds Mitchel, "the small spot gradually faded from the sight and was not seen at any subsequent observation."

Certainly these observations accord remarkably well with Mr. Williams's theory respecting the polar snows of Mars. The objections to the theory are found mainly in facts already mentioned. Thus it is difficult to understand how a sufficient quantity of the vapour of water should be present in the Martial atmosphere to produce the dark bands seen by Dr. Huggins, if the atmosphere itself (that is the permanent atmosphere) were so tenuous as the theory implies. It must, however, be noted that the tenuity of the atmosphere would encourage evaporation; in fact, the boiling point at the surface of Mars would be so low as 138° with Mr. Wiliams's assumption as to the atmospheric pressure, and lower still with mine. Nor does so great a difficulty arise as at first sight might be supposed from the fact that large Martial regions have at times seemed to be clouded over, since, in the first place, clouds would not be an unfrequent phenomenon in tenuous atmosphere; and under certain circumstances, as for example great atmospheric disturbances or the effects of such arctic catastrophes as Mr. Williams has described, there might be occasional extensions of dense, though perhaps shallow, cloud-layers, or heavy mists, over wide tracts of the surface

of Mars. The one great difficulty which, as it seems to me, would be fatal to Mr. Williams's theory, if demonstrably shown to exist, is the darkening near the terminator of the planet. It is possible, however, that this darkening may be shown to be merely relative. It is to be remembered that, assuming Mr. Williams's theory to be true, the region of evening or morning whitening would be very much less foreshortened at the time of corresponding quadrature than as seen when the disc is full. The obvious consequence of this would be that on the side towards the terminator there would be a much broader whitened border, and not only would the phenomenon be less noticeable on that account (since the narrowness of the white bordering is what renders it so remarkable), but the gradation of light would be much slower. Then, from the obliquity with which the solar rays fall on the parts towards the terminator, there is necessarily (whatever theory we adopt), a real defalcation of light there, and this defalcation may probably be more easily recognisable than the mere excess of light due to the whiteness of this part of the disc. In fact, passing from the centre of the illuminated half of Mars to the terminator, we have first the ruddy or greenish tints of the lands or seas, then a gradually increasing whiteness up to the absolute white of the hoarfrost covered region, then a gradual defalcation of light without change of colour; and the sole question is, Is the latter defalcation likely to be more or less recognisable by the telescopist than the deficiency of light in the middle of the disc on account of the ruddiness or greenness there? It is by no means certain what answer is to be given to this question. The subject has not, indeed, been specially studied by telescopists. When it has been studied with due photometric appliances, and under favourable circumstances (for which the present opposition-period of Mars affords an excellent opportunity) it may be possible to form a decided opinion on the exceedingly interesting and important suggestions made by Mr. Williams.

I shall not make many remarks upon the ordinary theory that the meteorological latitudes of Mars resemble those of our own earth, because this theory has been discussed at considerable length in my works referred to above. But there is one point on which I must make a few remarks. If we remember that the power of an atmosphere to increase the mean temperature depends in the main on its density at the mean level of the planet, we shall see that for Mars to have a climate such as that of our earth, there must be much more air above each square mile of the planet's

surface than there is above each square mile of the earth's surface. For the density of the air at the sea-level is proportional to the weight of the air above each unit of surface. For this weight, in the case of Mars, to be the same as in the case of the earth, the quantity of air above each unit of surface must be greater in the proportion of 1000 to 387, that being the proportion in which terrestrial gravity exceeds gravity at the surface of Mars. Taking 18 to 7 as sufficiently near, we have the following consequences if we assume that at the surface of Mars the atmospheric pressure is the same as on the earth. We have in the first place a coating of air, which is greater in quantity, square mile for square mile, than on the earth in the proportion of 18 to 7. But it must also be correspondingly greater in depth, for we know that on the earth the pressure is halved at a height of $3\frac{1}{2}$ miles, in other words that half the atmosphere lies below this height. At seven miles the pressure is reduced to one-fourth—that is, three-fourths of the air lie below this level: and so on. Now, in the case of Mars, the reduction proceeds in the same way, but at different heights. We must increase $3\frac{1}{2}$ in the proportion of 18 to 7 to obtain the height above the mean surface of Mars, at which the atmospheric pressure is reduced one-half. This gives nine miles as the elevation required. At a height of 18 miles, the pressure is reduced to one-fourth; and so on. Now on our assumption as to the actual quantity above each square mile of the surface of Mars, the region above the mean level of the planet to a height of 18 miles is occupied by air, having a mean density as great as that of the air below the height of seven miles from the terrestrial sea-level. Moreover, if we assume a height of 35 miles only as that to which the optically effective atmosphere of the earth extends, we get for the corresponding height in the case of Mars no less than 90 miles. Now, remembering that the diameter of Mars is but about 4400 miles, it seems clear that an atmosphere so deep as this should be telescopically recognisable.

But this is not all: if Mars had an atmosphere no denser at the sea-level than the terrestrial atmosphere, he would not have the same climate as the earth; for as we have seen the solar light and heat at Mars are reduced in the proportion of about 4 to 9 as compared with the solar light and heat at the earth. A very much denser, and therefore a very much deeper, atmosphere than that deduced above would be required to produce a Martial climate resembling our own; and even then, it may be questioned whether with his

relatively small ocean surface (here I refer to the actual proportion between the extent of land and water on Mars, and not merely to the extent of water surface in square miles), the atmosphere would be sufficiently vapour-laden to produce the required warmth. For it is to be remembered that dry air is almost perfectly diathermanous, as well for the luminous as for the obscure heat-rays, and that therefore the heat of Mars would be freely radiated away into space, unless the air were freely laden with aqueous vapour. It seems difficult to believe that Mars has an atmosphere so deep and dense as the conditions here considered appear to require.

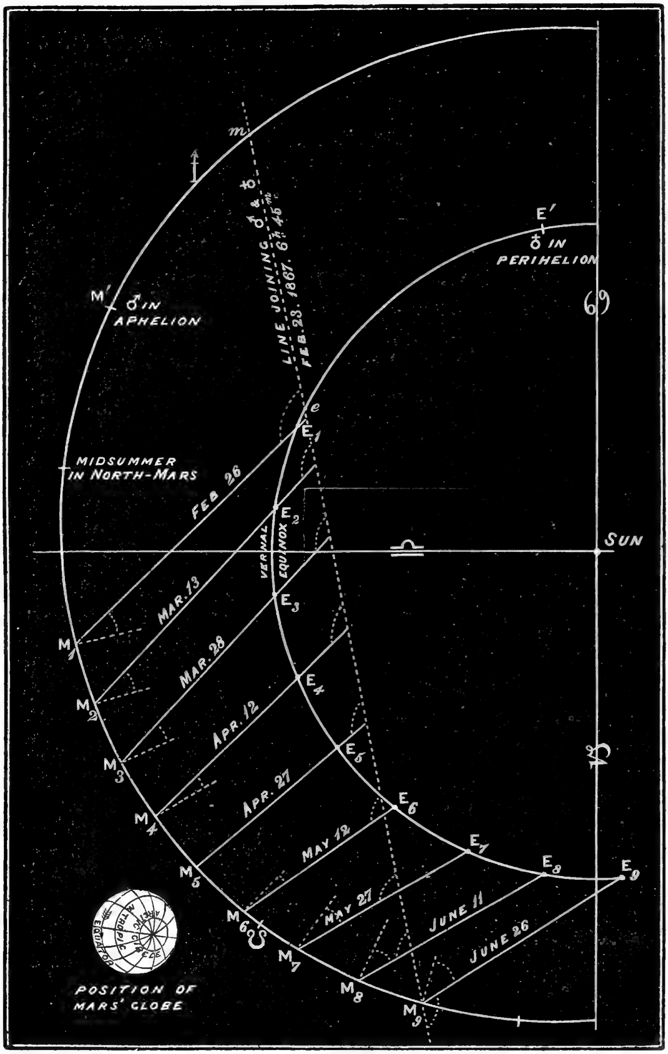
On the whole, I cannot but think that the balance of evidence is in favour of the theory that Mars has a relatively rare atmosphere, and that the various phenomena presented by the planet are to be explained in the way suggested by Mr. Williams.

The reader will perceive that a considerable degree of interest attaches to the study of Mars. We are by no means dealing with a planet whose physical habitudes have been thoroughly mastered and interpreted.

But, apart from these considerations, the present opposition of the planet is one which is peculiarly favourable to the investigation of the planet's condition. A reference to the accompanying figure (and to the plate illustrating this essay) will serve to show this.

In the first place, let it be noticed that when Mars is in opposition, on April 27, he is not far from the place where he is at his mean distance from the sun; for M′ is the place of his aphelion. So far, therefore, as the epoch of opposition is concerned, the present return of the planet may be regarded as having a medium value.

Let it next be noticed that the midsummer of the planet's northern hemisphere occurs when Mars is not far past his aphelion, and that the period illustrated by the figure corresponds to the summer months of North Mars. Now a peculiar interest attaches to this circumstance. Mars resembles the earth (at the present time, and for many years past and to come) in having his solstices near the aphelion and perihelion of his orbit; and the resemblance extends even to the circumstance that the summer of North Mars occurs when the planet is near aphelion, precisely as our summer in the northern hemisphere occurs when the earth is near aphelion. And precisely similar consequences follow from the relation in both cases. Our northern summer is mitigated by increase of distance from the sun, while


m
LINE JOINING ♂ & ♁
FEB. 23. 1867. 6h. 45m.
E′
♂ IN
PERIHELION
♋
M′
♂ IN
APHELION
e
E1
MIDSUMMER
IN NORTH-MARS
FEB. 26
E2
VERNAL
EQUINOX
♎
SUN
MAR. 13
E3
M1
MAR. 28
M2
E4
APR. 12
M3
E5
♑
APR. 27
M4
E6
MAY 12
E7
M5
E8
E9
M6
♈
MAY 27
JUNE 11
M7
JUNE 26
M8
M9
EQUATOR
TROPIC
ARCTIC CIR
POSITION OF
MARS' GLOBE

the northern winter is mitigated by the reduced distance of the sun; and, on the contrary, the summer heat and winter cold of the southern hemisphere are both intensified. Just so it necessarily happens in the case of Mars, but the effects are more marked, because of the greater eccentricity of the orbit of Mars. The heat received by Mars at mid-summer of his northern hemisphere is less than that received at mid-winter, in the proportion of about 7 to 10. Of course this is more than compensated, in north latitudes resembling our mid-temperate and subarctic zones, by the greater length of the summer's day and the greater height of the midday sun in summer. Nevertheless, the contrast between summer and winter must be most importantly reduced by the relation. On the other hand, the summer of the southern and winter of the southern hemisphere of Mars are intensified by the circumstance that more heat is received directly from the sun at the time of southern mid-summer than at the time of southern mid-winter, in the proportion of 10 to 7. The northern summer is also longer than the southern, to the following extent:—Counting from the vernal to the autumnal equinox the northern summer contains 371½ days, while the southern contains only 296½ days. Thus we have, in the northern hemishere, a long mild summer and a short mild winter; in the southern hemisphere, we have a short but (relatively) hot summer and a long and bitter winter.

It is manifest that, under these circumstances, we may fairly look for a great difference in the aspect of the northern half of the planet during the present opposition period, when the effects of the northern summer (counting still from equinox to equinox) are nearly at a maximum, and that presented during the corresponding opposition-period for the southern half of the planet,—the period, namely, including the opposition of 1864. Then the southern half had passed through its relatively intense summer, and there was a relatively rapid diminution of heat towards the mean heat at the equinox. Now the northern half has passed through its mitigated summer, and a relatively slow diminution of heat is taking place. As several excellent pictures of Mars were taken by Mr. Dawes, in 1864-65,* it will be possible to institute a comparison between the phenomena then observed and those which may be recognised on the present occasion.

It is next to be noticed that the present opposition-period

* The four best appear among the coloured illustrations of my "Other Worlds."

is singularly favourable for observing the gibbous phase of Mars after opposition. For it will be perceived that, even when the line joining the sun and Mars is immediately inclined to the line joining the earth and Mars,—as E_7M_7, E_8M_8, and E_9M_9,—the distance of the planet is not very much greater than when Mars is in opposition at M_5. Thus the disc of the planet, it will be seen (from the illustrative plate), diminishes much more slowly in size after opposition than it had increased before opposition. The telescopist should not lose this excellent opportunity for studying the way in which the disc seems coloured near the terminator. A careful comparison between the part of the disc near the terminator and on the opposite side cannot but prove most instructive. It will be observed that the terminator marks the place where morning is breaking on Mars (before opposition, of course, the terminator marks the place of Martial sunset); accordingly the occasion is favourable for determining whether, supposing there is whitish light near the terminator, that light is sharply defined towards the middle of the disc. Of course this can be done at any epoch of the opposition-period; but it can be best done near quadrature, because either the morning or evening part of the planet is then less foreshortened than at other times.

Next notice another circumstance. Whereas the motion of Mars on his orbit causes the solar elevation north of the Martial equator to continually diminish throughout the period dealt with in the figure,* the elevation of the *earth* north of the Martial equator does not change in the same way. It is easy to see why this is. We may regard Mars, during the opposition-period, with reference to its bearing from the earth when Mars is at M_1, his bearing from the earth is the same as his bearing from the sun when he is near M_6, and accordingly the elevation of the earth north of the equator-plane of Mars is nearly the same on February 26th as the elevation of the sun north of the same-plane on May 12th.† Then, as seen from the earth, Mars sways

* Precisely in the same way, of course, as in the case of the earth, as specially illustrated in my "Sun-Views of the Earth."

† The table towards the end of the present note gives the actual relations of the Martial globe, as well with reference to the circles and parallels of declination as to the sun. For the convenience of the reader who may care to test the results here tabulated, I give the formulæ and elements from which the table has been calculated.

The elements on which the determination of the axial position of Mars has been based are those given in No. 858 of the "Astronomische Nachrichten," in a paper by Dr. Oudemans upon the observations made by Bessel with the Königsberg heliometer, between the years 1830 and 1837. He gives (as quoted in a note by Mr. Hind in 1867)—

slightly forwards, for E_2M_2 is slightly inclined (and in that sense) to E_1M_1; hence, precisely as happens from the forward motion of Mars round the sun (in this part of his orbit), the elevation of the earth south of his equator-plane slightly diminishes. But from this position right onwards, until the position M_8 (or thereabouts), Mars is swaying backwards (around the earth); hence all this time the elevation of the earth south of the planet's equator-plane is increasing. And lastly, as Mars moves forwards round the earth, after passing M_8, the elevation of the earth south of the equator-plane slightly diminishes. These results are indicated in the table which is given in the foot-note.

Now it is easy to perceive how these results accord with the presentation of Mars in the nine projections of the illustrative plate. In projection 1 we see how the terminator, continued beyond its northern extremity (at the bottom of the projection), must pass farther from the pole than does

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	°	′	
Longitude of pole of Mars	349	1	for ecliptic.
Latitude	61	9	

Assuming these numbers to apply to 1834·0, we find—

	°	′
Longitude of ascending node of equator of Mars upon his orbit (λ′)	80	50·4
Obliquity of martial ecliptic (ι′)	27	16·5

And hence—

Ascending node of equator of Mars on the earth's equator (N)	47	33·9
Inclination (I)	39	55·6

For 1873·0 + *t* these values give—

$$N = 47^\circ\ 53' + 0{\cdot}50t$$
$$I = 39^\circ\ 43' - 0{\cdot}25t.$$

I have adopted these values in the computation of p and l in the accompanying table; p being the apparent inclination of the axis of Mars to the circle of declination, and l the elevation of the earth above the equator of the planet; using the following formulæ:—

Let α be the geocentric right ascension of Mars
δ „ declination „

and Q an auxiliary angle such that—

$$\tan Q = \tan i \, . \, \sin (\alpha - N),$$

then—

$$\tan p = -\frac{\sin Q}{\cos (Q - \delta)} \cot (\alpha - N)$$

and—

$$\tan l = \tan (Q - \delta) \cos p.$$

These formulæ are given by Mr. Hind in the note referred to above, and are the same as are used in the "Nautical Almanac" for determining the position and phase of Saturn's ring. (They are given in full, with others, among the explanations of the tables in my "Treatise on Saturn"). But in Mr. Hind's

the actual boundary of the disc. It is clear, moreover, that the upper or southern part of the disc is *viewed* more directly than it is illuminated; for, where the edge of the terminator is there seen, the solar rays are falling tangentially on the globe of Mars, whereas the lines of sight from the earth do not here fall tangentially. (Of course the same remark

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note, by an inadvertency, the denominator in the expression for $\tan p$ is written $\sin (Q-\delta)$. The following formulæ can be used, if preferred:—

$$\cos l \cdot \sin p = -\sin I \cdot \cos (\alpha - N)$$
$$\cos l \cdot \cos p = \sin I \cdot \sin (\alpha - N) \sin \delta + \cos I \cos \delta$$
$$\sin l = \sin I \cdot \sin (\alpha - N) \cos \delta - \cos I \sin \delta.$$

Moreover, if l' be the elevation of the sun above the plane of the ring, λ the heliocentric longitude of Mars, then, with sufficient approximation,—

$$\sin l' = \sin (\lambda - \lambda') \sin I'.$$

Strictly speaking, formulæ corresponding to those given at p. 229 of my "Treatise on Saturn" should be employed, viz., putting—

β = Mars' heliocentric latitude,

ν = longitude of ascending node of Mars' orbit on ecliptic,

and Ω' = arc from ascending node of Mars' orbit on ecliptic to ascending node of Mars' equator on his orbit.

Then assuming—

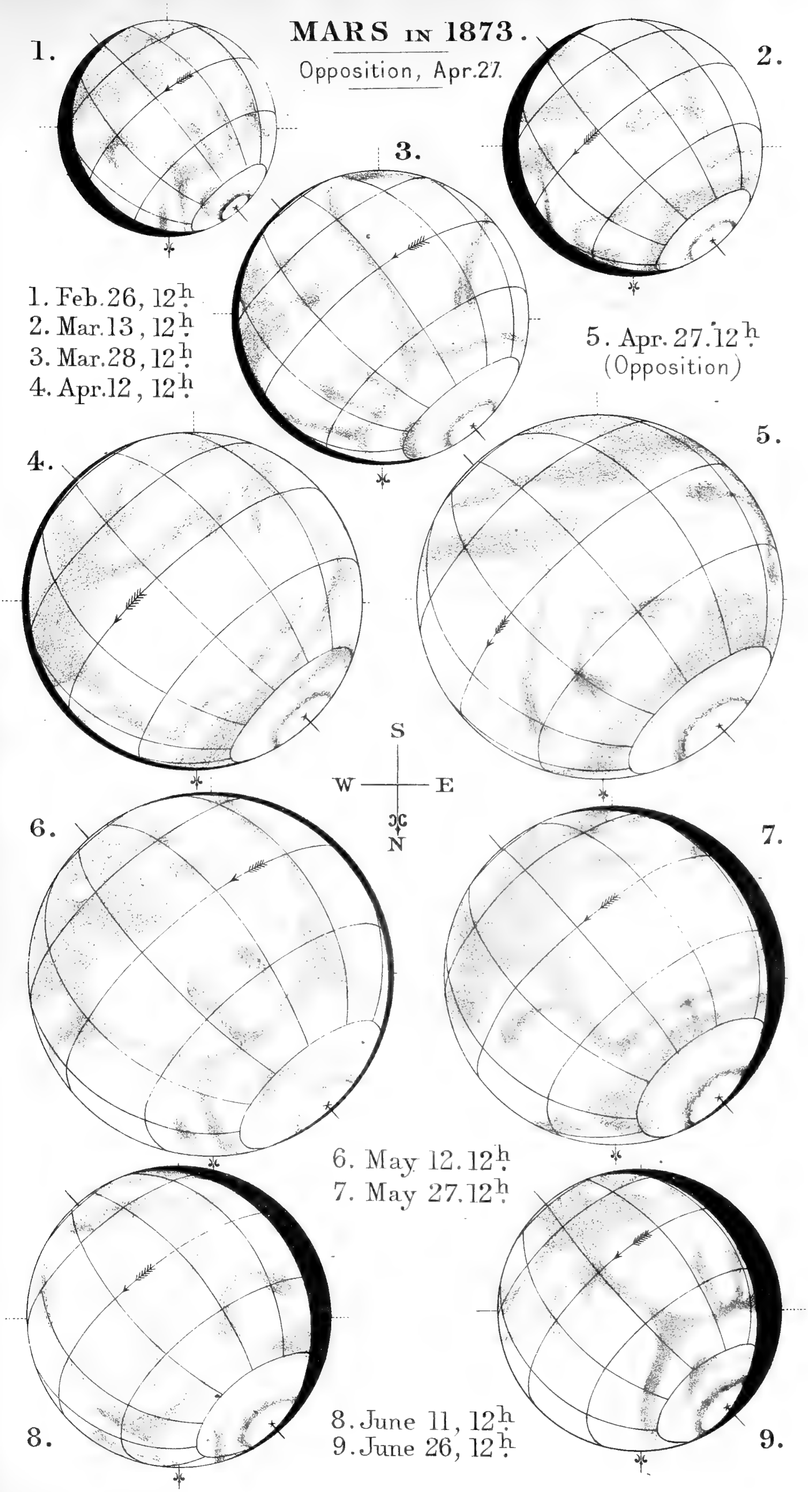
$$\cos \Psi = \cos (\lambda - \nu) \cos \beta,$$

we have—

$$\sin l' = \sin (\Psi - \Omega') \sin I'.$$

Date. Greenwich. Noon. 1873.	p.	l.	l'.	$\lambda - \lambda'$.
	° ′	° ′	° ′	° ′
Feb. 26	41 4 W.	15 7 N.	25 47 N.	108 21
Mar. 13	41 3	14 7	24 29	115 12
28	41 4	14 15	22 49	122 10
April 12	41 10	15 34	20 47	129 15
27	41 0	17 57	18 23	136 29
May 12	40 31	20 30	15 40	143 53
27	40 2	22 18	12 30	151 27
June 11	40 2	22 59	9 21	159 13
26	40 33	22 36	5 50	167 11

It will be seen that the value of p changes very little during the four months. Usually p changes largely. Thus in the opposition-period of 1866-7, p ranged in value between 9° 50′ and 21° 52′. The reason of the approach to constancy in the value of p during the present opposition is readily seen on a consideration of the figure given above. For we see that, viewed from the earth, Mars first slightly advances, then retrogrades through opposition, and then slightly advances. As this motion takes place along a part of the ecliptic where that circle is descending from the first point of Libra to the tropic of Capricorn, it follows that, so far as this motion is concerned, the apparent slope of the polar axis of Mars to a declination circle (west) at first slightly diminishes, then increases, and towards the end slightly diminishes again. This change depends simply on the inclination of different parts of the ecliptic to declination circles. But the apparent slope of the axis of Mars is also changing precisely as the opening out of his equator is changing (see column under l), being least when the opening out of the equator is greatest, and *vice versa*. So far as this cause of change is concerned, the slope of the axis first slightly increases, then diminishes, and towards the end slightly increases again. Comparing these with the changes due to the other cause, we see that the two changes are compensatory. Hence p remains very nearly constant.



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applies to every part of the terminator, but the point has been already considered for the middle parts). Now the result of this is, that the southern parts of Mars, where winter is in progress, are better seen than they would be if the line of sight from the earth were coincident with the line from the sun. Similar remarks apply to projections 2, 3, and 4, but to a gradually diminishing degree.

After opposition the reverse holds,—a fact of more importance, because it is the polar part of the planet which is now more directly viewed than illuminated. It is seen from the projections 6, 7, 8, and 9, how the terminator now passes between the north pole and the northern edge of the disc. It is obvious that the opportunity is thus an excellent one for studying the behaviour of the north polar snow as the summer months pass gradually on towards the autumnal equinox. This opportunity ought not to be lost by those who possess telescopes sufficiently powerful to distinguish the shape and dimensions of the polar snow-caps.

Lastly, it remains that I should make a few remarks on the features of the surface of Mars.

It will be understood that the projections in the illustrative plate are not intended to resemble pictures of Mars.* The land regions and oceans, for instance, are carried right to the very edge of the disc, whereas in reality they are concealed near the edge, under the white light already referred to. These projections are, in fact, masses of Mars, but an orthographic or natural projection, so that they show the various features as they would be seen if Mars were like a terrestrial globe and his aspect not affected by meteorological relations of any sort.

I may be permitted to point out that it was by means of constructions resembling those in the illustrative plate that

* The woodcut shows the method by which the areographic features of Mars, for the epochs indicated in the plate, have been determined from an observation of Mars made on February 23, 1867, at 6h. 45m. p.m., by Mr. Browning. (The hour in each case is midnight, Greenwich mean time). The picture of Mars then obtained is shown in Plate II. of my "Essays on Astronomy." Between the date of that observation and April 27, 1873, midnight, there is an interval of 194,850,900 seconds. Taking the rotation period of Mars as 88642·73 seconds, I find that the number of rotations of Mars amounts to 2198 + a rotation through 57°. I take the Kaiser Sea as 21° from the central meridian in Mr. Browning's picture (approaching the meridian), and the line joining the Earth and Mars on April 27 makes an angle of about 117° with the corresponding line on February 23, 1867. This obviously amounts to setting Mars 117° *back* in rotation. Thus, instead of 2198 Rot. + 57°, we have 2198 Rot. − 60°, or the Kaiser Sea 81° from the central meridian, instead of 21° as on Feb. 23, 1867, at 6h. 45m. The picture of Mars for April 27, No. 5 of the plate, corresponds with this result. The others have been obtained from similar considerations, account being taken in every case of the changing bearing of Mars from the earth.

I succeeded in interpreting the telescopic pictures of Mars obtained by Dawes, and in forming from them the stereographic and Mercator's charts of Mars which appear in my "Other Worlds" and "Essays on Astronomy" respectively. For every picture which he lent me or had published I constructed the proper orthographic projection, of suitable size, and carefully timed with reference to the actual rotation progress of Mars. It need hardly be said that the results were not found to be in strict accordance, simply because Mr. Dawes in drawing was not able to represent the features of Mars precisely as they were. Eye-judgments must always be, to some slight extent, faulty; and though some of his pictures must have been remarkably accurate (especially those taken in his later years), yet some slight discordances nevertheless existed. The charts, as finally drawn by me, present the features so that their shapes form a sort of mean between the various shapes which result from the separate drawings.

I believe it will be found that the telescopic study of the planet during the approaching opposition, with continual and careful reference to the accompanying projections, will enable any tolerably good draughtsman, possessed of adequate telescopic means, to improve our knowledge of the planet's features. It need hardly be said that, from the various projections given in the plate, the aspect of the planet at any time may be readily determined. The meridians marked on the planet are 30 degrees apart, and, since the planet rotates once upon its axis in 24h. 37m. $22\frac{3}{4}$s., it follows that he rotates so as to carry one of these meridians to the place occupied by the next forwards in a period of 2h. 3m. 7s. very nearly. This would be strictly correct if it were not for the circumstance that, as we see Mars from the earth, account has to be taken of the varying direction in which he is seen. For instance, comparing the position of Mars at M_1 in the figure with his position at M_9, it is manifest that it would be insufficient merely to consider so many rotations and parts of a rotation, in order to deduce his aspect at any given hour when he is near M_9. For the line from M_3 to the earth at E_9 is, as it were, swayed round from the direction occupied by the line from Mars at M_1 to the earth at E_1, and in a direction contrary to that of the planet's rotation; see the globe of the planet in the lower left-hand corner of the picture. And it is plain that this has precisely the same effect as though the planet had rotated so much farther forward.

But although in long intervals this is an important

consideration, it is not important in determining the aspect of the planet at any hour on any day intermediate to those corresponding to the projections of the plate. For we see from the figure that the lines E_1M_1, E_2M_2, &c., are in every case inclined at a small angle to their next neighbour lines. Moreover, by determining the aspect of Mars, from the nearest of the projections in point of time, we are sure of not having more than half even of this difference. Also, by means of a protractor, the angular change of the line of sight can be determined from the figure, and taken duly into account.

It is convenient to notice that at any given hour on any night the planet presents appreciably the same aspect as on the preceding night, 37m. 22¾s. earlier.

So far as the *shapes* of the parallels on the different projections are concerned, it is manifest that the change is too slight, from projection to projection, to introduce any difficulty. Nor will any draughtsman find any trouble in reducing or enlarging the scale to the proper dimensions, should he think this necessary. I believe that further explanation of these points is unnecessary, but to prevent any difficulty which may arise I will take an example:—Let us suppose the observer desires to know the aspect of the planet at 1 o'clock on the morning of May 1st (that is, in astronomical time, at 13h. on April 30th). In this case three days and one hour have elapsed after 12h. April 27th, the epoch of projection 5. Now three Martial days are equal to three of our days and 1h. 52m. 8¼s. So that Mars at 1 on May 1st will be rotated as much forward, compared with the aspect observed in projection 5, as corresponds to 52m. 8¼s.; but from meridian to meridian in the projections corresponds to an interval of 2h. 3m. 7s. So that each meridian in projection 5 must be shifted forwards by a less distance than that separating it from the next meridian, in the proportion that 52m. 8¼s. is less than 2h. 3m. 7s., or that 3128¼ is less than 7387. This proportion may be taken as the same as 3 to 7. So that, if we trace the parallels and circular outline of disc from projection 5, and shift each meridian (this also can be done in tracing,—that is, there is no occasion to pencil the meridians as they actually are) forwards by three-sevenths of the space separating it from the next to the left, we have the required meridians and parallels. The features can then be drawn in from projection 5, being carried forward by the same amount as the meridians. In most cases the application of this method requires the features to be completed from one of the other

projections. But there is no difficulty in doing this, because the connection between the different projections is very readily traced. Thus, although in comparing 1 and 2 we find nothing to guide us,—for, in fact, the hemispheres shown are almost exactly opposite,—yet projection 3 at once supplies features lying to the right of those shown in 1; and projection 4 at once supplies features lying to the left of those shown in 1. So projections 4 and 5 supply features lying to the right and left of those shown in projection 2. And so throughout the series.

V. THE KENT'S HOLE MACHAIRODUS.

By W. Pengelly, F.R.S., F.G.S.

THE late Rev. John MacEnery, of Torquay, and Kent's Hole, near the same town, rendered each other famous. Those who knew the former tell us that the truth is by no means exceeded in the following eulogy on his gravestone, near the belfry door, in Torre churchyard :—" He had an heart formed for friendship; and, whilst as a clergyman he conciliated all classes by his amiable manners, he inspired respect as a scholar by the vigour of his understanding, his polished taste, and varied learning." Nevertheless, he is now almost exclusively known as the first who made any important discoveries in the great natural mausoleum near which he lived and died.

Though Kent's Hole appears to have been known from time immemorial, and was one of "the lions" of the district in the 18th century, and though fossil bones were discovered in it in 1824, first by Mr. Northmore, and afterwards by Sir Walter C. Trevelyan, it was not until Mr. MacEnery commenced his researches in 1825 that palæontologists and archæologists became aware of its great importance.

Amongst his reputed discoveries none have attracted so much attention as (1st) the inosculation of relics of human industry with bones of extinct mammals, and (2nd) the occurrence of remains of the animal formerly known as *Ursus cultridens*, but now as *Machairodus latidens*; and long after some of the best thinkers had accepted the former they remained sceptical respecting the latter. The difficulty was as follows :—Remains of Machairodus had been found at Epplesheim, in Germany, and in the Val d'Arno, in Italy,

but in deposits considerably older than those in Kent's Cavern; moreover, no indication of the genus had been found in any other part of Britain. So strongly was this difficulty felt by one eminent palæontologist that he was wont to express the opinion that MacEnery had obtained some of the specimens found in Italy, that in his collection they had got mixed with the Kent's Hole fossils, and that he had incorrectly, though in perfect good faith, ascribed them to his favourite cavern. Recently, however, all the facts of the case have been collected and published,* and no doubt now remains of the perfect correctness of MacEnery's statements. They have, moreover, been confirmed by the Committee at present exploring the cavern, who had the good fortune to discover there a tooth of the same species.

In this paper, which is to be devoted to the Kent's Hole Machairodus, the following points will be discussed;—

1. The evidence that MacEnery found Machairodus in the cavern.

2nd. The remains of it which he discovered.

3rd. Its era.

4th. Its place in the zoological series.

I. The Evidence that MacEnery found Machairodus in Kent's Cavern.—Mac Enery states that he commenced his systematic "diggings at the close of 1825," † and, as will presently be shown, that he found the fossils in question in January, 1826. The earliest known published mention of the discovery appears in the following notice, of fossils and a communication, received by Professor Jameson from Dr. Buckland, printed in the "Edin. Phil. Journ." for April, 1826.‡ "Professor Buckland has lately sent to Professor Jameson, for the College Museum, several specimens of bone from the hyæna's den at Kent's Hole, near Torquay, all of which he considers as bearing the most decided marks of teeth and gnawing upon them. . . . [In the cavern.] There are also *album græcum*, as at Kirkdale, and stumps of gnawed horns of deer, and the bony bases of horns of rhinoceroses, but no *horns of this animal*, although more than a hundred of its teeth have been already found; also the teeth of many infant elephants, numberless bones of horses, elks, deer, and oxen; and gnawed bones of hyænas, with their single teeth and tusks; also the teeth and tusks of

* Trans. Devon. Assoc., iii., p. 483, 494, iv., p. 467.
† *Ibid.*, iii., p. 444.
‡ Vol. xiv., p. 363-4.

bears, tigers, wolves, and foxes, *and of an unknown carnivorous animal, at least as large as a tiger, the genus of which has not yet been determined*."*

It is perhaps worthy of note that, as the "Philosophical Journal" was published quarterly, no mention of a discovery made in January, 1826, could have appeared in its pages earlier than in the number for April of the same year—that from which the foregoing quotation has been taken.

If any doubt exists as to the great "unknown carnivorous animal" being Machairodus, it will probably be removed by the following extract from a letter sent by Dr. Buckland to Mr. MacEnery, and of which a copy is preserved in the archives of the Yorkshire Philosophical Society;—

"Lyons, 14th March, 1826.

"My dear Sir,—I should have forwarded the enclosed† from Paris had I not waited to visit a spot in Auvergne, where they have recently discovered a deposit of animals similar to those of Kent's Cave, in a bed of diluvial sand and gravel.

"The resemblance is still more striking from the fact of there being among them the teeth of your *unknown animal*,‡ which turns out to be the *Ursus cultridens* of Cuvier, which had till now been found only in the Val d'Arno. There is a complete skull of this bear in the collection at Florence.

* * * * *

I have sent the gnawed fragments you gave me to Scotland, and trust that ere this opposition in that quarter will have ceased."

It cannot be doubted that the "unknown animal," which turned out to be *Ursus cultridens* mentioned by Buckland in the letter just quoted, was the "unknown carnivorous animal" he spoke of in his communication to Jameson.

It was well known that Mr. MacEnery intended to publish by subscription an account of his researches. A copy of his prospectus, now before us, shows that it was to be illustrated with thirty plates representing the objects in the natural size, and that specimens of the plates had been prepared, and were on view. At his death, however, in 1841, the work had not been published, nor could his manuscript be found, and the plates appear to have been lost sight of. Subsequently, the manuscript was recovered, and seventeen of the lithographed stones were also found. The work was

* These italics are not in the original.

† Letter from the Baron Cuvier to Rev. J. MacEnery.

‡ These words are not italicised in the original.

by no means ready for the press, but in 1859 Mr. E. Vivian, of Torquay, published a compilation from it; and through the liberality of Mr. F. Buckland, whose property the stones had become, he was allowed to have some proof impressions taken for its illustration. The plates are distinguished with letters of the alphabet, from B to T inclusive, J and O being missing. The first sixteen contain figures of the remains of animals, and the seventeenth of flint implements. They all state that the specimens represented on them were found in "Kent's Hole, Torquay;" fourteen of them that they were "lithographed from nature by G. Scharf;" one, F, that it was delineated by "Mary Buckland," and lithographed by "G. Scharf;" the remaining two, H and I, are silent on this point; twelve give the information that the specimens were found by Rev. J. McEnery; one, C, by Rev. L. P. Welland, whilst the remaining four give no information on the subject.

Plate F contains seven figures representing, in the natural size, different aspects of at least three distinct canines, and has the following label:—"Mary Buckland del., G. Scharf, lithog., Nat. size. Teeth of Ursus cultridens. Found in the Cave of Kent's Hole, near Torquay, Devon, by Revd. Mr. McEnery, January, 1826, in diluvial mud, mix'd with teeth and gnaw'd bones of rhinoceros, elephant, horse, ox, elk, and deer, with teeth and bones of hyænas, bears, wolves, foxes, &c." It is the only plate in the series which was drawn by "Mary Buckland," or bears the date on which the specimens were found, or names the animals with whose remains they were mixed. In short, there was a full recognition of the fact that the discovery was regarded as one of importance. It may be, too, that scepticism respecting it was foreseen and provided for, so far as was possible.

The plate, as we have seen, states that the teeth were found in January, 1826, and this harmonises with the facts, that according to the records of the Geological Society of London, one of them was presented to that body by Mrs. Cazalet, February 17th, 1826, and that Sir W. C. Trevelyan, as he has been so good as to inform us, was at Torquay in 1826, about the end of February and beginning of March; that on the last day of the former he spent some hours excavating in the cavern, and that one of the teeth of Machairodus was given to him by Mrs. Cazalet (he thinks), and not by Mr. MacEnery.

In his manuscript, the whole of which was published in 1869, exactly as he left it,* Mr. MacEnery mentioned the

* See Trans. Devon Assoc., iii., p. 191-482.

discovery of the Machairodus remains no fewer than seven distinct times, and states that he found them in a branch of the cavern known as the Wolf's Cave.*

Of the foregoing statements the following is briefly the sum :—Mr. MacEnery commenced his systematic researches at the close of 1825. In January, 1826, he discovered in the Wolf's Cave, teeth of an animal, which he submitted to Dr. Buckland, who, like himself, was ignorant of their true character. Very shortly after their discovery, he gave two of them to Mrs. Cazalet, his friend and co-religionist, † who presented one of them to the Geological Society of London, on the 17th February, 1826, and the other to Sir W. C. Trevelyan about the end of that month or the beginning of the next. Prior to 14th March, Dr. Buckland, describing the contents of the cavern to Professor Jameson, mentioned the occurrence of "an unknown carnivorous animal at least as large as a tiger, the genus of which had not been determined." Subsequently, Dr. Buckland visited Paris, when he submitted the teeth, or more probably casts of them, to the Baron Cuvier, and on March 14th, 1826, when writing to Mr. MacEnery from Lyons, he informed him that his "unknown animal" had turned out to be the *Ursus cultridens;* adding, and this to one with whose palæntological knowledge he was well acquainted, that previously it had been found only in the Val d'Arno.

If the written statements of Mr. MacEnery, Dr. Buckland, and Sir W. C. Trevelyan be insufficient to establish the proposition that Machairodus remains had been found in Kent's Cavern, we may well despair of evidence. Happily, however, the proposition was confirmed on July 29th, 1872, as has been already stated, when the Committee at present charged with the exploration of the cavern by the British Association discovered another tooth of the same species.

II. *The remains of Machairodus which MacEnery discovered in the Cavern.*—Respecting the remains of Machairodus he found in Kent's Hole, MacEnery says "it is scarce, only five teeth having been found." Proceeding to describe one of the teeth, he says, "Its form is semi-lunar, compressed, and tapering to a point like a blade ; and along the course of the enamel, which occupies nearly one-half of its entire length and assumes a fine edge, it is delicately dentated—*vide* plate F, figs. 1, 2, 3, exhibiting different views of the most perfect tooth. The curved fang was snapped off, and

* *Ibid.*, pp. 240, 243, 294, 368-70, 371-2, 421, and 456-7.

† Mr. MacEnery was the Roman Catholic Priest at Torquay.

the hollow of the tooth disclosed, which, with its unworn point, shows it to have belonged to a young individual. The other teeth represented in the same plate are truncated at their apex, and despoiled of their posterior serrature, while the anterior indenture is entire. . . . The appearance of the blunted apex of the teeth bespeaks the effect of violence. The part is not worn down and polished as is the case with teeth employed in bruising vegetables, but broken sharply off as if from the act of piercing its foe. . . . The enamel is longitudinally situated, and the base of the fang is distinguished by dotted lines in strong relief?

"Judging from the wear of the apex and the solidity of the fangs, three of the specimens belonged to adult individuals. They are all gnawed at their base, and the young one cracked across."*

In a subsequent passage he adds:—"In addition to the canines, I have lately discovered in the same bed a small tooth about an inch long. The internal face of the enamel is fringed with a serrated border. This tooth is distinguished further by two tubercles or protuberances at the base of the enamel, from which the serration springs, and describes a pointed arch on the internal surface, *vide* figs. 8, 9.† The body of the tooth in this specimen is not compressed but rounded. Whether this belongs to an inferior species of *U. cultridens*, or is simply the incisor anterior to the canine of *U. cultridens*, I am not able to pronounce with certainty."‡

This latter tooth was subsequently identified, figured, and described by Professor Owen as the right external upper incisor of his *Machairodus latidens*.|| MacEnery's statement respecting its size must have been based on a rough guess, not on actual measurement, for, instead of being "about an inch long," it measured, according to Professor Owen's figure, 1·97 inches in length, in a straight line, from the vertex of the crown to the base of the fang.

What has become of the incisor appears to be entirely unknown; but the five canines have all been traced. One of them, as we have seen, was presented to the Museum of the Geological Society of London; and Sir W. C. Trevelyan has recently presented his specimen to the Museum of the

* *Ibid.*, pp. 369, 370.

† These figures are not known. From the fact that he does not specify the plate in which they occur, it seems probable that they were to be added to plate F, the last he had previously specified, and that in which the canines of Machairodus were represented.

‡ *Ibid.*, p. 370.

|| Hist. Brit. Fossil Mammals, &c., 1846, pp. 177, 182.

Geological Survey in Jermyn Street, London. The remaining three were in MacEnery's collection at his decease, and were disposed of at the sale of his effects. Dr. Battersby, late of Torquay, says in a letter to us on the subject, "There was a card sold at Mr. MacEnery's sale with three teeth (serrated on each side), and marked *Ursus cultridens*. These were purchased jointly by Dr. Phillips and myself. After the sale was over, Mr. König, of the British Museum, came to me and said he had been particularly anxious to have bought them, but had not observed they were on the card until after it was knocked down. Dr. Phillips and I then agreed to give him one for the Museum. Dr. Phillips sent his to the Museum at Oxford. . . . The third I forwarded to Lord Enniskillen, with a number of other teeth, &c., I had purchased for him." . . . Lord Enniskillen subsequently sent his specimen to the Museum of the College of Surgeons, London. It is unnecessary to add that the specimens are carefully preserved in the five museums named above.

As will subsequently be shown, the upper and lower canines of Machairodus are so very dissimilar as to render it quite safe to assert that the Kent's Hole specimens all belonged to the upper series, and thus to render it certain that at least three individuals of *Machairodus latidens* found their way to Britain; and, from what has been stated, that two of them were adults and perhaps aged, whilst the third was young.

The following questions, however, have lately been raised respecting the actual number of teeth found:—

1. Were there not more than five canines?
2. Were there not two incisors?

1. *The Number of the Canines.*—During the progress of his researches, Mr. MacEnery sent specimens of the cavern remains and casts of the rarer fossils to various museums, and amongst others to London, Paris, York, and Bristol. His present to York included "a correct cast of one of the serrated teeth of the *Ursus cultridens* of Cuvier," and was accompanied by a descriptive letter, dated May 3, 1826, enclosing copies of the letters which, as already mentioned, he had received from Cuvier and Buckland. In the Report on MacEnery's present and communication, drawn up by the Rev. W. V. Harcourt, President of the Yorkshire Philosophical Society, and laid before that body, it is stated that "M. Cuvier . . . . found one of the specimens . . . to be the canine tooth of that species of bear which he has named *Ursus cultridens;*" and from this passage it has been

inferred that an actual tooth, and not a cast merely, formed part of the present sent to Paris. There is nothing, however, in either of the letters to justify this inference. On the contrary, MacEnery's list of the specimens he sent to York closes with the remark that "Similar collections to the one now forwarded have been transmitted to Cuvier for the Paris Museum, to Professor Buckland for the London Geological Society, and to Bristol;" thus rendering it at least probable that, as to York, a "correct cast" only was sent to Paris. That casts *were* sent thither is quite certain, for, when visiting the museum, May 2nd, 1872, we made a special and successful search for them; and whilst they were before us, made the following notes:—"In the Palæontological Museum, in the Jardin des Plantes, there are three plaster casts of teeth of Machairodus from Kent's Cavern, two canines, and one incisor. The crown of the first is broken.

"The following three labels accompany them:—

"Label 1. 'Felis cultridens d'Angleterre, Ost. Pl. xvii.'

"Label 2. 'Modèles en Plâtre de 2 canines superieures donnés, par Mr. Mac-Enri.'

"Label 3. 'Modèles en Plâtre d'un Incisive sup. par Mr. Mac-Enri.'"

There was certainly no actual tooth of Machairodus from Kent's Hole in the collection; and when it is remarked that the casts presented in 1826 had been carefully preserved for forty-six years, it may be concluded that less care would not have been bestowed on an original tooth, and that there is nothing to warrant the belief that more than five canines—the number mentioned by MacEnery—were found in the cavern.

2. *The Number of the Incisors.*—We have already seen that according to MacEnery's statement he found one incisor, and that when describing it he referred to figs. 8 and 9, which do not occur in any of his series of plates which have been recovered, but were perhaps intended to be introduced into "Plate F"—his Machairodus plate.

In 1869, several plates were presented to the Torquay Natural History Society by gentlemen who had obtained them from an executor of Mr. MacEnery, whose property they formerly were; many of them were copies of the seventeen already mentioned, but two of them were new ones belonging to the same series—plates O and U. Besides these were some that certainly did not represent Kent's Cavern fossils, and had nothing whatever to do with the series. There was one, however, a drawing in Indian-ink,

containing five figures, two of them representing different aspects of a portion of the upper jaw of a horse, whilst the remaining three were those of two incisors of Machairodus, in all respects closely resembling the incisor of *Mach. latidens* from Kent's Cavern, figured, as already stated, by Professor Owen. Besides the figures, there is nothing on the plate but the words "J. Scharf del, 1837."

On the strength of these three figures it has recently been concluded that MacEnery found two incisors in Kent's Hole,* but, in reply, it may be stated that there is nothing to indicate that the plate in question belonged to the cavern series, or represented Kent's Hole fossils; and that, if it did, it could not have been the plate to which he referred, as it contains but five figures, whilst his reference was to "figs. 8 and 9." In short, it seems impossible to deny that the evidence that MacEnery found more than one incisor is certainly very inconclusive.

It is perhaps worthy of remark that Professor Gervais, in his *Zoologie et Palæontologie Françaises* has the following observation under "*Machairodus latidens*":—"Fossil from England in Kent's Cavern. I cite this species among our fossils of France from a single incisor found near Du Puy (Haute Loire) by M. Aymard, in soil probably diluvian, and which he has communicated to me; this tooth quite resembles, by its crenulated edges, that which was discovered in England by Mr. MacEnery, and that of De Blainville and M. Owen."† Is it possible that the figures in the plate under notice are those of the two Machairodus incisors, found one in Kent's Hole, by Mac Enery, the other near Du Puy, by Aymard, and placed side by side for comparison?

III. *The Era of the Kent's Cavern Machairodus.*—It has been already stated that one of the difficulties in the way of the acceptance of MacEnery's reputed discovery, was that the Machairodus remains found in continental Europe belonged to deposits of higher antiquity—those of Epplesheim and Auvergne being miocene, and those of the Val d'Arno pliocene; and though the difficulty was at least partially removed by the fact that the Kent's Hole fossils, though of the same genus, belonged to a distinct species, it was still held to be so remarkable as to require

* See "The British Pleistocene Mammalia." By W. Boyd Dawkins, M.A., F.R.S., and W. Ayshford Sanford, F.G.S., Part iv., Pal. Soc., 1872, pages 185—188.

† *Op. cit.*, 2nd edit., 1859, p. 231.

confirmation. At present, however, the chronological chasm has been almost, if not entirely, bridged over by M. Aymard's discovery of a tooth of the same species near du Puy, and the disinterment in Buenos Ayres of an almost complete skeleton of *Mach. neogaeus*, to be described more fully in the sequel, which, according to Dr. H. Burmeister, was the contemporary of the Megatherium and other pleistocene forms.

In discussing the question immediately before us, it will be necessary to give a brief description of the successive deposits in Kent's Hole:—First, or uppermost, was a very dark coloured mud, from 3 to 12 inches in depth, and known as the *Black Mould*. Beneath it was a floor of stalagmite, commonly of laminated and granular structure, and termed the *Granular Stalagmite* or *Floor*. Next below was an accumulation of bright red loam, with about 50 per cent of angular fragments of limestone, and designated the *Cave-Earth*. In certain parts of the cavern this rested on a second or lower floor of stalagmite, of highly crystalline texture, in some places upwards of 12 feet thick, and termed the *Crystalline Stalagmite* or *Floor*. Under this lay, so far as is at present known, the lowest and oldest deposit of the cavern, consisting of sub-angular and rounded pieces of dark red grit, embedded in a sandy paste of the same colour; the whole being known as the *Breccia*. Large coherent masses of the breccia, as well as of the granular stalagmite, occurred in various branches of the cavern incorporated in the cave-earth; thus showing that prior to the introduction of the latter they were more important formations than they are at present.

All these deposits contained bones and teeth of animals. In the uppermost, or black mould, they were those of existing species, but in all below it remains of extinct as well as of recent forms presented themselves. In the cave-earth and the granular stalagmite formed on it, but especially the former, the ordinary cave mammals were very abundant; the hyæna being the most prevalent, but followed very closely by the horse and rhinoceros. Remains of megaceros, ox, deer, badger, mammoth, and bear were by no means rare; whilst those of fox, lion, reindeer, and wolf were less prevalent; and those of beaver, glutton, and Machairodus were very scarce. In the lower deposits—the crystalline stalagmite and the breccia—remains of animals were less uniformly distributed. In some places none were met with throughout considerable areas, whilst in others they formed 50 per cent of the entire deposit; but, so far as is at present

known, they were exclusively those of bears. Not only were there no bones or teeth of the hyæna, but none of his coprolites, nor were any of the bones broken after his well-known pattern, or scored with his teeth marks.

The bones found in the black mould, or most modern deposit, differed much in specific gravity from those in the lower accumulations, and were generally so light as to float in water. The remains in the cave-earth and breccia had lost their animal matter, and adhered to the tongue when applied to it, so as frequently to support their own weight; but those from the latter were much more mineralised than the specimens found in the cave-earth.

The following general statements may be of service here:—

1. Animal remains were much more abundant in the mechanical deposits than in the stalagmites.

2. The period represented by the *Breccia* and *Crystalline Stalagmite* may, so far as the cavern is concerned, be termed the *Ursine period;* the deposits having yielded remains of bears only.

3. The period of the *Cave-Earth* and *Granular Stalagmite* may be denominated the *Hyæna period;* the hyæna remains being restricted to these deposits.

4. The period of the *Black Mould* may be called the *Ovine period;* remains of the sheep having been found in but not below this accumulation.

5. The bones of each period were distinguishable by their mineral condition; those in the *Black Mould* being much lighter, and those in the *Breccia* being more mineralised, than the remains yielded by the *Cave-Earth*.

Some of the masses of breccia occasionally incorporated in the cave-earth were found to contain bones possessing all the characters of such as were met with in the undisturbed breccia; and a few fossils, easily distinguishable by their mineral condition, had certainly been dislodged from the breccia or older deposit, and re-deposited in the relatively modern cave-earth, without being attended by any discoverable portion of the accumulation in which they had been primarily interred. Hence the question, "Is not this the History of the Kent's Hole Machairodus?" is one which presents itself when considering the era of that species, and which presses for a distinct and definite reply. Indeed, it has recently received a qualified answer in the affirmative,* but which appears to us not to be borne out by the evidence.

The following is the substance of MacEnery's statements

* See Brit. Pleist. Mammals, *Pal. Soc.*, Part iv., 1872, p. 191.

having a bearing on this question :—No teeth of Machairodus *were* found in those parts of the cavern in which the deposit yielded remains of bears only ; in other words, in the breccia. This he regarded as a very noteworthy fact, as he supposed the animal to have been a species of bear. They were met with in the branch known as the *Wolf's Cave*, mixed with the teeth and bones of hyænas, and the gnawed bones of rhinoceros, elephant, and the other ordinary cave-earth mammals. Though some of the remains mixed with them bore marks indicative of contusion, they, though "delicately-edged," bore no such indications. The fang of one of the canines had been broken across, and all the others had been gnawed.* Having carefully examined some of the canines, we can confirm the statement that they are gnawed ; and can add that their mineral condition is that of specimens from the *Cave-Earth*, not the *Breccia*.

Had the teeth in question been derived from the breccia and re-deposited in the cave-earth, it might have been expected that some remains of the same kind would have been met with amongst the immense number of fossils found in the undisturbed original deposit; but instead of this, neither MacEnery nor the British Association Committee, whose uninterrupted and systematic labours have now extended over eight years, met with the least trace of Machairodus in the breccia. Again, the present explorers carefully re-examined all the deposit broken up, but not removed, by MacEnery in the Wolf's Cave, and they excavated there to a depth greater than that to which he restricted himself; but they neither met with any detached bone or tooth having the mineral character indicative of fossils from the breccia, nor any trace of the older deposit, either as incorporated fragments or *in situ*. When to these facts—important, though negative—it is added that the teeth under notice have the mineral condition betokening the cave-earth, and that they have not suffered abrasion or contusion, which it is scarcely possible to suppose they would have escaped had they undergone dislodgment, transportation, and re-deposition, especially when the very delicate serration of their edges is borne in mind, a very strong case seems to be made out in favour of the proposition that *Machairodus latidens* was a member of the *Cave-Earth* fauna. There is, however, another and a most important fact. As already stated, the fangs of the canines are gnawed ; the work, in all probability, of the hyæna—an animal which seems to

* See Trans. Devon. Assoc., Part iii., pp. 240, 243, 294, 370, 371, and 457.

have been master of the cavern during the cave-earth era, but of which no indications whatever have been found in the breccia.

The conclusion to which the foregoing facts concur in pointing, was confirmed by the incisor found, as already stated, by the British Association Committee, July 29th, 1872. It lay in the uppermost foot-level of cave earth, below the granular stalagmite, and below it were teeth of hyæna, horse, and bear; in short, the evidence shows that the Kent's Hole Machairodus belonged to the cave-earth, or hyæna period; and, should any facts hereafter present themselves proving it to have been a member of the fauna of the Breccia, they will in no way disturb this conclusion, but will simply prove that, like the cave bear, *Machairodus latidens* belonged to both eras.

IV. *The Place of Machairodus in the Zoological Series.*—Remains of animals, all now recognised as belonging to the genus Machairodus, have been found in Italy, Germany, various parts of France, England, Brazil, Buenos Ayres, and the Sewalik hills in India, and have been described under the names of *Ursus cultridens, U. etruscus et cultridens, U. cultridens arvernensis, U. cultridens issidorensis, U. depranodon, Felis cultridens, F. cultridens etuariorum, F. megantereon, F. megantereon et cultridens, F. palmidens, Machairodus cultridens, M. latidens, M. palmidens, M. neogaeus, Megantereon brevidens, M. macroscelis, Hyæna neogaea, Smilodon populator, Munifelis bonaêrensis, Stenodon,* and *Agnotherium.*

Professor Nesti was the first to describe the large falciform canines from the Val d'Arno, and in 1824 he exhibited them to Cuvier, who referred them to the genus *Ursus*, under the name of *Ursus cultridens.* In 1828, M. Bravard found a complete skull in Auvergne, with the falciformal canines *in situ*, and proved that the jaw was like that of the cat's; hence he proposed that the animal should be called *Felis megantereon et cultridens.* In 1833, Dr. Kaup, in his description of the Epplesheim fossils in the Darmstadt collection, pointed out that the compressed canines had neither the longitudinal grooves nor the two ridges which characterise feline canines, that no carnivorous quadruped had the enamelled crown of the canine so long, or its concave edge so serrated, and that in these respects they resembled the teeth of the *Megalosaurus*,—an extinct species of gigantic land-saurian,—and he proposed a new genus, *Machairodus* (sabre-toothed) for the extinct species to which they belonged.

Besides the upper tusks, Kaup was acquainted with those of the under jaw, which are comparatively very small; and, not thinking that they belonged to the same animal, assigned them to another genus, which he named *Agnotherium*.

Dr. Lund, digging in the bone caves of Brazil, found joints of toes and molars which he thought those of a hyæna, and described them under the name of *H. neogaea* in 1839; subsequently, being convinced by the singular tusk that the animal belonged to a distinct genus, he made it known under the name of *Smilodon populator*. His *Smilodon*, however, was the *Machairodus*.

In 1846, Professor Owen, describing the Kent's Hole Machairodus, says, "In this extinct animal, as in the *Machairodus cultridens* of the Val d'Arno, and the *M. megantereon* of Auvergne, the canines curved backwards, in form like a pruning-knife, having the greater part of the compressed crown provided with a double-cutting edge of serrated enamel; that on the concave margin being continued to the base, the convex margin becoming thicker there, like the back of a knife, to give strength. Thus, each movement of the jaw, with a tooth thus formed, combined the power of the knife and saw, whilst the apex, in making the first incision, acted like the two-edged point of a sabre. The backward curvature of the full-grown teeth enabled them to retain, like barbs, the prey whose quivering flesh they penetrated. . . . One of the largest of the canines of the *Machairodus cultridens* from the Val d'Arno measures 8·5 inches in length along the anterior curve, and 1·5 inches in breadth at the base of the crown. The largest of the canines of the *Machairodus* from Kent's Hole measures six inches along the anterior curve, and one inch two lines across the base of the crown; the English specimens are also thinner or more compressed in proportion to their breadth, especially at the anterior part of the crown, which is sharper than in the *M. cultridens*. These differences are so constant and well marked as to establish the specific distinctness of the large British sabre-toothed feline animal; for which, therefore, I propose the name of *Machairodus latidens* [broad-toothed, sabre-toothed.]"*

It is obvious that Professor Owen acquiesced in separating the animals under discussion from the typical *Felidæ*, that he adopted the generic name of Machairodus proposed for them by Professor Kaup, and that he regarded the Kent's Hole form as specifically distinct from that of the Val d'Arno. The last decision was objected to by the late

* Brit. Foss. Mam., p. 179, 181.

Dr. Falconer, who says, "The length of the Italian tooth is 8·5 in., and the breadth of the crown at the base 1·5 in., while the corresponding measurements of the English specimens are 6 and 1·2 in. The breadth of the English tooth ought to be only 1·06 in., were the proportion to the length the same as in the Italian. Owen says these differences are constant and well marked. But are they sufficient for a distinction of species, or are the materials sufficiently abundant to affirm their constancy? I think not. In my opinion, the English *Machairodus latidens* is probably the same as the Italian *M. cultridens*." *

It has always struck us that in this passage the case is not stated with the author's well-known usual fairness. Professor Owen *named* his species, no doubt, from the greater relative breadth of the crown of the canine, but he *separated* it from the Italian, not on this account only, but also because of the difference in actual dimensions, the greater relative compression of the English specimens, and the sharper anterior edges of their crowns. Be this as it may, Messrs. Boyd Dawkins and Ayshford Sanford, having stated Dr. Falconer's objection, say, "We consider the British *Machærodus latidens*, Owen, to be distinct from the *M. cultridens* of the Continent;"† and they call attention to Professor Gervais's statement that the incisors in the almost entire skull found in Auvergne by M. Bravard, and admitted by all to be M. *cultridens*, are not crenulated as in *M. latidens*.‡

In 1844, Dr. Franz Xavier Muniz found near Lujan, 12 leagues west of Buenos Ayres, the almost complete skeleton of a beast of prey, a contemporary of the Megatherium, Mylodon, Glyptodon, Taxodon, and Mastodon. Finding nothing like it in Cuvier's *Ossem. Foss.*, he described it under the name of *Munifelis bonaërensis*, in the "Gaceta Mercantil" of 9th Oct., 1845.

It proved, however, to be the skeleton of a species of Machairodus, and in October, 1865, Dr. Herman Burmeister, who in 1861 took the management of the State Museum of Buenos Ayres, succeeded in securing the specimen for his museum, through the munificence of Mr. William Wheelwright, contractor of the Argentine Central Railway from Rosario to Cordova. Dr. Burmeister proposes publishing a full description in the "Anales del Mus. publ. de B.A.," but in the meantime he has sent to his friends in Germany a brief notice of the

* Palæont. Memoirs, 1868, vol. ii., p. 459.
† Brit. Pleist. Mam., Part IV., 1872, p. 187.
‡ Zoologie et Palæontologie Françaises, 2nd. Ed., 1859, p. 231.

most important parts of the construction. This paper was "specially printed from the treatises of the Natural History Society at Halle," and is accompanied by a figure, from a photograph of the skeleton as it now stands in the Museum, which shows its excellent preservation. We propose incorporating a very condensed summary of Dr. Burmeister's paper, of which, so far as we are aware, no notice has appeared in British journals, for though the skeleton is not that of *Machairodus latidens*, it is beyond all comparison the most perfect specimen of the genus which has been found, and cannot fail to throw considerable light on his British relative.

The country between the small towns of Lujan and Meroedes forms an oval trough, running from S.W. to N.E., in the midst of which is the little river on which both towns are situated. It is peculiarly rich in well-preserved skeletons of gigantic animals, most of which are on the level of the water, or a little above it.

As the species is the same as that found in Brazil by Dr. Lund, who, apparently not aware of the researches of Dr. Kaup, described it under the name of *Hyæna neogaea*, in 1839, or six years before Dr. Muniz described his specimen as *Munifelis bonaërensis*, Dr. Burmeister has done the former an act of justice by acknowledging the priority of his specific name, and calling the creature *Machærodus neogaeus*.

Everything about the body resembles that of the *Felidæ*, and but for the skull and teeth no one would be able to distinguish it from that genus. Notwithstanding the great size of its tusks, the animal did not reach the size of the existing lion or tiger, and the cave-lion (*Felis spelæa*) was considerably larger.

The following measurements show that relatively to the length of the body, exclusive of the tail, its skull was shorter than that of either the lion or tiger:—

	Skull.	Body.	Ratio of Skull to Body.
Lion	12·5 in.	66 in.	189 : 1000
Tiger . . .	11·6 in.	60 in.	194 : 1000
Mach. neo. . .	13·0 in.	72 in.	186 : 1000

Though, as shown above, the skull is actually longer, it is much smaller than that of the tiger. In the enormous development of the *crista occipitalis* it resembles the hyæna. The face is of great breadth, which is probably due to the astonishing size of the upper tusks, and the long oval form of the relatively small eye orbits.

The under jaw of *Mach. neo.* is considerably smaller than that of the lion, and only a little longer, but at the same time decidedly much narrower than that of the jaguar; but the palate is much broader than that of the lion or tiger, at least in front. The following measurements, in inches, of the length of the under jaw, from the front edge of the tusk to the back part of the edge of the neck, in some of the larger *Felidæ* and *Mach. neo.* may be of interest here:—Cave-lion (*Felis spelæa*), 11·4; Lion (*F. leo*), 9·4; Tiger (*F. tigris*), 8·3; Ounce (*F. onca*), 7·9; and *Mach. neo.*, 8·7. The under jaw of Machairodus is known with certainty by the forestanding edge-comb of the chin on each side, beside which lies the great canine of the upper jaw. It seems to indicate that the point of this tusk could not be hidden under the lips when the mouth is closed, though the upper lip was much broader and more fleshy than that of the existing *Felidæ*.

The Buenos Ayres skull contains three upper and two lower molars on each side. The foremost of the lower series is wanting, and there is no trace of an alveolus. The number and formation of tubercles on them is quite like those of the feline animals.

The following are the dimensions of the upper canine:—Length of the crown 5 inches, of the gum 1 inch, of the root 4·5 inches; total length in a straight line 10·5 inches. The under tusk is surprisingly small in comparison, and scarcely larger than the upper outer incisor. Both the upper and lower canines are devoid of the longitudinal furrows which the tusks of the real *Felidæ* possess—two upon each side of the upper, and one on the outer side of the lower.

The external upper incisors, like the lower canines, are conical, bluntly pointed, slightly bent inwards, and bluntly three-cornered. In the *Felidæ* the outer incisors, especially in the upper jaw, are much larger than the inner ones, which are of equal size; whilst in the lower series a difference of size is perceptible between the inner and the middle ones on each side. In *Machairodus neogaeus* the difference of size between the three on each side, in each jaw, is much more considerable, and the gradual increase from inwards to outwards is not to be mistaken. The teeth of the upper and lower jaws also harmonise more with each other both in form and size—each one of the lower series being a little smaller than the corresponding incisor in the upper. The following measurements of the crowns clearly show the proportions of the several teeth:—

	Upper.	Lower.
Inner incisor . .	0·63 inch . . .	0·31 inch
Middle do. . .	0·79 ,, . . .	0·39 ,,
Outer do. . .	0·98 ,, . . .	0·83 ,,
Canine do. . .	5·00 ,, . . .	1·02 ,,

In the form of their crowns, the difference between the incisors of the *Felidæ* and *Machairodus* is very decided; for, instead of being chisel-shaped as in the former, every one in the latter is thoroughly conical, extends to a simple rounded point, and is slightly incurved throughout, the point itself standing perpendicularly. Close to the point are two more or less sharp edges, which run along both sides of the crown and get thick and callous below. At the bottom of the crown they turn inwards, get weaker, and approach each other at an angle, which includes a blunt and scarcely perceptible tubercle. These edges have also slight notches corresponding to those of the under tusk.

The conical form of the incisors, as well as the lancet-like upper canines, shows in a high degree the bloodthirsty nature of the Machairodus. Assuming, as very probable, that the objects of his bloodthirstiness were the great *Edentata* of South America—the Megatherium, Scelidotherium, Mylodon, and Glyptodon—it is clear that a sharp long-pointed set of teeth was necessary for killing animals covered with a hard coat of mail, and only a beast of prey like the Machairodus could have been able to kill them. These large animals did not possess the means of active defence. Even the powerful claws of their fore-legs were of no use. For defence, they had only their clumsy figures and coats of mail. The Machairodus, therefore, required the long sharp tusks and pointed incisors to be able to take hold of and kill his prey. The tusk of a tiger or lion could not possibly have penetrated the skin of a Mylodon or Glyptodon. It harmonises well with this description that the South American species of Machairodus possessed such great upper and relatively small lower tusks; as it was only there that the coated gigantic animals existed. In Kaup's species the upper tusks were smaller and the lower ones larger; and *Machairodus latidens*, as represented by Owen, differs still more from *M. neogacus*. The incisor figured by Owen has a thicker, but not a shorter, crown than that of the same tooth in the Buenos Ayres skeleton. This shows a much less disproportion in *M. latidens* in the extent of the incisors and tusks, and enables us to show this characteristic as a necessary consequence of a difference of construction for their food.

No part of the skeleton of *Machairodus neogaeus* differs so much as the head from the corresponding part in the existing *Felidæ*. The neck has a length of 15·25 inches. The *atlas* is shorter and a little broader, but not stronger, than that of the tiger; and is much inferior to that of *Felis spelæa*. Its form approaches that of the hyæna. The dorsal vertebræ are fourteen in number, the lumbar six, and the pelvic three. The tail is entirely wanting, but there are indications that it was smaller than in the existing large *Felidæ*, and probably not larger than that of the lynx.

The breast-bone and ribs are perfectly like those in the genus *Felis*. The former consists of nine pieces of bone, with a tenth, or terminal one, of cartilage. There are fourteen pairs of ribs, the first being 6·3 inches long, almost everywhere equally broad, and a little compressed; the second is thinner, and the succeeding ones get much thinner upward, thicker below, and teminate in a knob-like swelling. They increase in length to the seventh, which, like the three following it, is 11·4 inches long; after this they decrease to the fourteenth, which, like the first, measures 6·3 inches. How many of them were *false* has not been ascertained.

The bones of the extremities, taken singly, closely resemble those of the *Felidæ*, but when united it is seen that the forearm and lower leg are short in proportion to the upper arm and thigh. This will be clearly apparent in the following table, where the lengths are given in inches:—

	Mach. neo.	*Felis spelæa.*	*F. tigris.*	*F. domestica.*
Scapula	12·99	(?)	9·84	3·42
Humerus . . .	14·96	14·96	12·70	4·01
Radius	10·63	13·78	11·02	3·94
Manus	10·63	(?)	11·02	3·54
Metacarp. med. .	3·54	5·39	4·25	1·27
Pelvis	13·78	(?)	12·70	4·33
Sacrum	4·29	5·04	2·76	0·79
Femur	14·96	16·85	14·17	4·72
Tibia	9·84	(?)	12·70	4·72
Calcaneum . . .	4·33	5·33	4·13	1·22
Metatars. med. .	3·94	5·55	4·96	1·97

Whilst the lower bones of the fore-limbs are thus comparatively short, they are much stouter than those of the existing *Felidæ*. The bones of the lion, the most robust of the genus, scarcely reaches them.

It is clear that an animal like Machairodus, possessing such capacities for securing its prey, required very powerful

claws; and these excite astonishment by their size and solidity, especially on the inner toes of the fore paws. Even the same bones in *Felis spelœa*, as figured by Schmerling, are only a little longer, whilst the toes are much larger.

The shortening of the fore-limbs is much greater than that of the hinder. From the size of the scapula, the arm and hand of the Machairodus might have been expected to exceed those of the tiger, especially as the upper arm is much longer and thicker; but whilst the living tiger has a shorter shoulder-blade and a shorter upper arm, it has a longer fore-arm and paw, and the bones are much thinner than those of *Machairodus neogaeus;* hence, the strength of the animal is much less. The tiger is quicker and more versatile, but his power of beating down and grasping his prey is certainly less than that of Machairodus was.

Though in the hind limbs the difference is less marked, the lower leg was 5·12 inches shorter than the thigh, whilst in the tiger the difference is but 1·47 inches. The foot, notwithstanding, is almost of equal length in the two animals, and the heel of Machairodus was even longer than that of the tiger, thus proving the greater power of the former in its hinder extremities also.

There is no further difference between the number, position, and size of the hand and ankles of Machairodus and those of the living *Felidæ* than that all the small bones of the former are much stronger. It is the same with the bones of the toes, which, and especially those of the thumb of the fore-paw, are of extraordinary solidity and size.*

Palæontologists are at present acquainted with the following species of the genus *Machairodus* :—

M. cultridens,	found	in	Italy, Germany, and France.
M. latidens,	,,	,,	England and France.
M. palmidens,†	,,	,,	France.
M. sivalensis,	,,	,,	India.
M. neogaeus,	,,	,,	Brazil and Buenos Ayres.

* See Bericht über ein Skelet von Machœrodus, im Staats-Museum, zu Buenos Aires, von Dr. HERM. BURMEISTER. Halle: Druck und Verlag von H. W. Schmidt. 1867.

† See GERVAIS, *op. cit.*, p. 231.

VI. ATMOSPHERIC LIFE GERMS.

LORD Bacon in the "Novum Organum" (Book II., Aphorism 13), says, "All putrefaction exhibits some slight degree of heat, though not enough to be perceptible to the touch : for neither the substances which by putrefaction are converted into animalculæ, as flesh and cheese, nor rotten wood which shines in the dark, are warm to the touch." He thus gives as a definition of spontaneous generation the conversion of substances, such as flesh and cheese, into animalculæ. The joke of Dr. Johnson on Tom Davies, a bankrupt bookseller, who took to authorship, that he was "an author generated by the corruption of a bookseller," is evidently a hint as to his connection with Grub Street through an illusion to the popular belief.

The first recorded facts undermining the old belief in "spontaneous generation," were those of Redi, published in 1638, leading to the first exact experiments in closed vessels of Needham in 1745, and of Spallanzani in 1765; the experiments with air purified by heating of Schwann, and with air passed through oil of vitriol of Schultze in 1837; the proof that the solid particles of yeast alone can cause fermentation by Helmholtz in 1844; Schrœder and Dusch's experiments with air filtered through cotton-wool in 1854; and the repetition of the foregoing and complete investigation of the subject by Pasteur in 1862. The object of this paper is to make these last experiments more widely known; unfortunately they must be stripped of detail, and thereby robbed of much of their strength of argument. Few persons are familiar with the mode of experimenting, the facts observed, and the remarkable chain of evidence afforded by these most carefully-executed, most complete, and therefore most trustworthy, experiments.

Pasteur's Microscopic Examination of the Solid Particles Diffused in the Atmosphere.

The question which Pasteur first set himself to answer was, Is it possible to gain an approximate idea of the relation a volume of ordinary air bears to the number of germs that the air may contain? Let us see what means were taken to determine the number and the nature of floating particles diffused in the air.

By means of a water aspirator air was drawn from a quiet street, and also from the gardens of the Ecole Normale, in Paris, at some distance from the ground, through

a tube containining a plug not of cotton-wool, as in the experiments of Schrœder, but of soluble pyroxyline, such as is used for making collodion. The amount of air aspirated in a given time was accurately measured, and after a sufficient interval the soluble cotton plug was removed and treated with its solvents, alcohol and ether. After allowing the dust to subside in a tube, the collodion was syphoned off, and more alcohol and ether added to effect the perfect removal of the collodion. The completely-washed dust was placed on a microscope slip and examined in a drop of water. By ordinary methods the action of different reagents, such as iodine water, potash, sulphuric acid, and colouring matters on the particles was tried. This process disclosed the fact that there is in ordinary air a variable number of corpuscles, ranging in size from extreme minuteness up to the diameters of 0·01 m.m. to 0·015 m.m.; some translucent particles of a regular shape so closely resemble the spores of the most common fungi that the most able microscopist could see no difference in them. The corpuscles were evidently organised, resembling completely the germs of the lowest organisms, and so diverse in size and structure as to belong without doubt to very various species. The soluble cotton used was previously tested and found to contain no residue insoluble in alcohol and ether beyond a fibre or two. By exposing a plug of pyroxyline for twenty-four hours to a current of air passing at the rate of a litre the minute after a succession of fine spring days, it was found that many myriads of organised corpuscles were collected.

Experiments with Heated Air.

Although it appears there are in air organised corpuscles in great numbers which are indistinguishable from the germs of the lowest organisms, is it really a fact that amongst these there are particles capable of germination? This interesting question was answered in a conclusive manner. Firstly, the facts announced by Schwann were firmly established, although they had previously been attacked by Mantegazza, Joly and Musset, and Pouchet. The solution, sealed up in flasks, was one extremely liable to change; its composition was—

Water	100 parts.
Sugar	10 ,,
Albumenoid and mineral matters from yeast	0·2 to 0·7 parts.

Boiled for two or three minutes, and then placed in contact

with air previously heated to redness, not a single doubtful result was obtained, although repeated at least fifty times; not a single trace of any organised production was seen even after eighteen months, keeping at a temperature of 25° to 30° C.; while, if the liquid be left to ordinary air for a day or two, it never fails to become filled with bacteria or vibriones, or covered with mould. The experiment of Schwann applied to this sugar solution is, therefore, of irreproachable exactitude. Schwann, however, did not always succeed so well as he wished, and the experience of Mantegazza and Pouchet was at variance with his general conclusions; even Pasteur himself in some experiments failed to preserve his liquids. These are the particular instances:—Five flasks of 250 c.c. capacity, containing 80 c.c. of the sugar solution, were boiled, and during ebullition sealed up. The points were broken under mercury, and pure gases in all cases but one let into the flasks. Organisms were found in every case after four days. In all these experiments, as in those likewise of Schwann, which were contrary to the result of his first experiment with extract of meat, it was the mercury that introduced the germs. In making such experiments with a mercury trough, preservation of the liquid will not always succeed, even if it succeeds sometimes. If the sugar solution be replaced by milk and treated by either of the methods above described, the milk putrefies. These results, so different and contradictory, find a natural explanation further on, but so far they are facts of a troublesome nature.

Germination of the Dust which exists suspended in the Air, in Liquids suitable to the Development of the Lowest Organisms.

The facts ascertained so far are:—

1. That there exist suspended in the air organised corpuscles exactly like the germs of the lowest organisms.

2. That sugar solutions with the liquor from beer yeast, a fluid extremely alterable in ordinary air, remains unchanged and limpid, without even giving rise to infusoria or fungi, when left in contact with air previously heated.

The question now arises, how is it possible to sow an albuminous sugar solution with germs collected by means of pyroxyline in the manner already described?

Taking a flask containing such a sugar solution kept at 25° to 30° C. for one or two months unchanged, in contact with previously heated air, the sealed-up end is connected by means of a caoutchouc tube with one part of a T tube, while another is in connection with an air-pump, and a

third with a platinum tube heated to redness. Between the T tube, however, and the flask is a wide tube containing a very narrow one within it, holding a plug of gun-cotton, through which a large volume of air has been passed. The tap in connection with the heated platinum tube was closed, and the one in connection with the air-pump opened; after exhausting air was admitted through the red-hot platinum, the tap was closed, and the air again pumped out, fresh air being admitted through the heated tube; this was repeated three or four times. The stop-cocks were then closed, and the sealed beak of the flask was broken within the india-rubber connection; the plug of gun-cotton was shaken into the liquid, after which the flask was sealed up again. All experiments so performed resulted in the liquid, after three or four days' exposure to a temperature of 25° to 30° C., decomposing, and being found to contain bacteria, vibriones, and fungi, just exactly like those in flasks exposed to ordinary air. There was no difference in the length of time requisite for the change, the forms of life occurring, or the nature of the change resulting in flasks so treated, and those with the same liquid exposed to common atmospheric air. These experiments can scarcely be surpassed for beauty in their arrangement, or for the importance and clearness of the evidence they afford. Yet thinking that it might be objected that the gun-cotton had given rise to the changes produced, Pasteur made use of plugs of asbestos, and found a like result; but when the plugs of asbestos were heated red-hot previous to being put into the flasks, the liquids remained unchanged in every case, and so constantly and with such perfect exactitude after an immense number of trials did the results remain the same, that the experimenter himself was astonished.

Extension of previous results to other very alterable Liquids—Urine, Milk, and Albuminous Sugar Solution mixed with Carbonate of Lime.

The facility with which urine exposed to the air becomes altered, and the change which takes place is well known. It becomes turbid and alkaline, sometimes filled with bacteria, or covered with patches of mucor or *Penicillium glaucum*. Often there is formed, when the temperature is not higher than 15° C., a pellicle consisting of a remarkable mucor closely resembling *torula*, but which is believed by Pasteur to be a different species. It consists of transparent cells, often without a nucleus, and considerably smaller than the cells of beer-yeast. There is also present in urine, when

alkaline from the carbonate of ammonia resulting from the changed urea, a peculiar fungus in necklace-like groups, and this organism Pasteur is fully persuaded is the cause of urea being converted into carbonate of ammonia. An interesting observation was made with regard to the turbidity of liquids, which generally is the first sign of alteration; this is caused not merely by the presence of minute organisms, such as bacteria, but by their movements in the liquid; for when they are dead they settle to the bottom of the vessel, and the liquid becomes clear again. Many flasks of urine were treated in the manner already described—that is to say, they were boiled, and heated air was admitted to them. After preservation for months at 25° to 30° C. without change, plugs of asbestos through which air had been drawn were introduced; and then in cases where the liquid was alkaline, strings of this peculiar fungus were found invariably, and crystals of ammonio-magnesian phosphate were deposited. It was observed that *Bacterium termo* appears in a liquid before any other organism. This infusorium is so small that it would be impossible to distinguish its germ; but even if the appearance of its germ were known it would be still less possible to recognise it among the various particles of organised dust collected from suspension in the atmosphere.

In experimenting with milk boiled in flasks and exposed to heated air, it was found that generally in from eight to ten days, but in one case after so long a time as a month, the milk was found to be curdled. Microscopic examination showed that the whey was filled with vibriones, often of the species *Vibrio lineola*, and bacteria. The air of the flasks showed that the oxygen was replaced by carbonic acid; yet swarms of these vibriones were living in an atmosphere without oxygen. The most important observation which leads to an explanation of the extraordinary behaviour of milk in these experiments, is the fact that no mucor, torula, or penicillium—nothing but bacteria or vibriones—were found in the liquid. The obvious conclusion is, that these organisms or their germs are not destroyed by a temperature of 100° C. when the heated liquid which serves to develop them enjoys certain properties. To test this supposition, the milk was boiled under pressure, so that the temperature was raised during ebullition to 110° C., and then heated air was admitted, of course at the usual atmospheric pressure; flasks treated in this way were kept an indefinite period without the production of any life whatever. The milk preserved its flavour, its odour, and all its

properties. Sometimes a slight oxidation of fatty matter took place, as could only be expected in such a considerable body of air; this was proved by an analysis of the air. In such cases the milk had a slightly suety taste. But what condition prevents the development of vibriones in sugar solutions and urine when heated to 100° C.? It is the fact that they contain a trace of acid. Milk is an alkaline liquid. If a liquid of the following composition:—

Sugar	10 grms.
Yeast water	100 c.c.
Carbonate of lime . . .	1 grm.

be boiled in flasks at 100° C., filled with heated air and sealed up and left to itself at 25° to 30°, in from two to four days it becomes turbid from vibriones, which have a very lively motion. It was found that a species of mucor after a time covered the surface of the liquid. It seems, therefore, that under these particular conditions, that the germs of this cryptogam had resisted the temperature of boiling water. An important confirmation of these experiments regarding the failure of a temperature of 100° C. to destroy certain germs here follows. Milk which had been preserved some months had a plug of asbestos presumably containing germs introduced into it by the manner already described; it was sealed up, and the flask was then plunged into boiling water; in eight days bacteria and vibriones were found in swarms. It was further discovered that 108° was too low a temperature to effect the preservation of these liquids.

It cannot be too forcibly impressed on the reader by what means and with what success Pasteur demonstrated the fact of myriads of organisms occupying comparatively small volumes of air. This is a point to which his detractors have willingly made themselves blind; they tell us the organisms are few in number without any experimental proof; while, on the other hand, Dr. Angus Smith and Mr. Dancer estimated that there were $37\frac{1}{2}$ millions of organisms, many of which were recognisable, in 2500 litres of Manchester air.*

Another Method for showing that all the Organisms produced by previously heated Infusions have for their origin the particles which exist suspended in ordinary Atmospheric Air.

Says Pasteur, "I believe it to be rigorously established in the preceding chapters that all the organised productions

* Air and Rain, p. 505.

of infusions previously heated, have no other origin than the solid particles which are always carried in the air and left deposited constantly upon everything. Could there still remain the least doubt of this in the mind of the reader, it will be dissipated by the experiments I will now describe."

The experiments consisted in placing in glass flasks the following liquids, all of which are very changeable in contact with ordinary air, yeast liquor, sugar solution and yeast liquor, urine, beet-root juice, and infusion of pears; the flasks were then drawn out so as to have a long neck with many bends in all directions. The liquid is boiled for some minutes, while the steam escapes plentifully from the open neck; the flasks are then left to themselves without being sealed, and, strange to say, though the air enters, the liquid may be preserved for an indefinite period—an interesting fact for those who are accustomed to make experiments of such a delicate nature as this subject requires. There is no fear of transporting these flasks from place to place, or submitting them to the varying temperature of the seasons; the liquids show not the slightest alteration in taste or smell; they are truly specimens of Appert's food-preserving process. In some cases there was a direct oxidation of the matter, a purely chemical process. But it has already been shown how this action of oxygen was *always limited when organised productions were developed in liquids.* The explanation of these new facts is, that the air on first entering comes in contact with water vapour at the temperature of 100° C., and is so rendered harmless; what follows enters but slowly, and leaves its germs or particles of active matter in the moist curvatures of the tube-neck. After remaining many months in a warm place, the necks of the flasks are cracked off by a file-mark without other disturbance, and in twenty-four hours to thirty-six or forty-eight, fungi and infusoria make their appearance in the usual manner.

The same experiments can be made with milk, but then the milk must be boiled under pressure; milk has been kept for months in these open flasks without change at a temperature of 25° to 30° C. The production of organisms can always be started in these flasks by briskly shaking the liquid or by sealing during ebullition, and after cooling allowing the air to enter suddenly by breaking the point of the tube.

Many such flasks, exhibited at the Academy of Sciences, were preserved with their contents unchanged for eighteen months, although extremely prone to decomposition.

"The great interest of this method is, that it unquestionably proves that the origin of life in infusions which have been boiled is solely due to solid particles suspended in the air. Neither a gas, divers fluids, electricity, magnetism, ozone, things known or hidden causes, there is absolutely nothing in ordinary atmospheric air which, failing these solid particles, can be the cause of the putrefaction or fermentation of the liquids which we have studied." It has so far been definitely proved by Pasteur, and stated in the following manner:—

"1st. That there are constantly, in ordinary air, organised particles which cannot be distinguished from the true germs of the organisms found in infusions.

"2nd. When these particles and the amorphous *débris* associated with them are sown in liquids, which have been previously boiled and which remained unchanged in air previously heated, there appear in these liquids exactly the same forms of life as arise in them when they are exposed to the open air."

"Such being the case, could a partisan of spontaneous generation wish to uphold his principles even in the face of this double proposition? He might, but then his argument would necessarily be of the following kind, of which I leave the reader to judge for himself. There are in the air, he might say, solid particles, such as carbonate of lime, silica, soot, fibres of linen, wool, and cotton, starch granules . . . and besides these organised corpuscles having a perfect resemblance to the spores of the Mucedineæ or the germs of Infusoria. I prefer to attribute the origin of Mucedineæ and Infusoria to the first amorphous substances rather than to the second."

This has actually been asserted. Could there be more eccentric reasoning? Reasoning it is not. That question is beyond the pale of argument, to which common sense dictates the answer.

It is not exactly true that the smallest quantity of ordinary Air gives rise in an Infusion to the Organisms peculiar to this Infusion. Experiments on the Air of various Localities. Inconvenience of employing Mercury in Experiments relative to Spontaneous Generation.

If the smallest quantity of air in contact with an infusion gives rise to organisms, and these organisms are not of spontaneous origin, then it follows that in the minute portion of air there must exist a multitude of the germs of very different organisms; in such numbers, too, that, as Pouchet

says, the air would be so loaded with organic matter as to form a thick fog. Strong as this reasoning is, it would be still stronger if it were shown that different forms of life are derived from different germs: this may be so, but it has not been proved.

Experimental proof of this statement, the error in which lies in gross exaggeration, was made by sealing up during ebullition flasks of 250 c.c. capacity containing about 80 c.c. of various liquids. On breaking the points of these flasks in certain noted places, the air entered with a rush into the empty space, carrying the germs along with it; after re-sealing, the flasks were placed in a warm situation and any change noted. In some cases the decomposition followed, and the production of the usual forms of life; in other cases the flask remained as if they had been filled with heated air, quite unchanged. In two experiments made in the open air after a slight shower in the month of June, both resulted in the production of organisms; in four others, after a heavy rain in the same place, two of the flasks had their contents remain unchanged for at least thirteen months afterwards. These experiments were made, it is easily seen, in an agitated air, but Pasteur carried his labours into the cellars of the Paris Observatory, where the air is quite still except when agitated by the movements of the experimenter, and in that region below the surface of the earth where the temperature is unaffected by the changes of the seasons. It is to be expected that air, in which there is so little to cause its disturbance, would have deposited on the ground the germs which at one time floated in it. A greater proportion of flasks therefore, if opened and re-sealed in such an atmosphere, should have their contents preserved. Out of ten experiments made under such conditions with yeast water, in only one was any living thing found; while eleven experiments made in the court-yard of the observatory at a distance of 50 centimetres from the ground, and at the same time, rendered in every case the usual forms of life; a modification of these trials was made by letting air into flasks of liquid at various mountain heights. Eighty-three flasks, prepared in the manner already mentioned, were experimented on: twenty of these were filled up with air at the foot of the heights which form the first plateau of the Jura; twenty others on one of the peaks of the Jura, 850 metres above the sea-level; and the remaining twenty were carried to Montanvert, near the Mer de Glace, at an elevation of 2000 metres. The result was, that of the twenty opened on the lowest level, eight contained organisms; of the twenty

on the Jura, five only contained any; and lastly, of the twenty filled at Montanvert, while a strong wind blew from the deepest gorges of the Glacier de Bois, one only was altered. The method of opening the flasks was to hold them above the head, with the point turned from the wind, and by a pair of iron forceps, which had just been heated in a spirit-lamp flame, the point was broken. The drawn-out point had been previously scratched with a file and heated; otherwise particles of dust adhering to the glass would have been carried into the liquid by the in-rush of air.

A remarkable and interesting fact connected with these experiments was, that on one occasion Pasteur opened his flasks, and, on account of not being able to see the flame of his lamp against the brilliancy of the snow, it was impossible to re-seal them; the flasks were necessarily carried back to the little inn at Montanvert to be closed up. Everyone of these flasks contained organisms after keeping for a short time. On the glacier then, there are no germs in the atmosphere, but at the neighbouring inn the air warms with life, and life from all parts of the world, brought by the travellers. On opening the flasks they were held above the head, so as to prevent the possibility of germs attached to the person being deposited in them.

Explanation of the Cause of Failure of the Experiments in which Mercury is used.

Flasks containing liquids which had been kept for a great length of time were connected with an air-pump and a red-hot platinum tube: after repeated exhaustion and re-filling with heated air, the communication was made between the flask and the platinum tube, and a globule of mercury taken out of a mercury trough in the laboratory, which had previously been introduced into the connecting-tube of india-rubber, was made to roll into the flask; on re-sealing and keeping for a few days, fermentation ensued in every case, just as certainly as when the asbestos plugs and the adhering germs were sown in similar liquids. This case leaves no doubt regarding the cause of failure of experiments in which the liquid comes in contact with mercury by the flasks being broken under the surface of the quicksilver.

There are other facts which Pasteur established, of great interest and importance in connection with the nutrition of ferments, mucors and vibriones. Instead of experimenting on milk, urine, or solutions containing the liquor from yeast,

he made use of such an infusion as the following; that is to say, a mixture of perfectly definite chemical substances:—

Pure water	100 parts
Sugar-candy	10 ,,
Tartrate of ammonia . .	0·2 to 0·5 part
Ashes of yeast	0·1 part

On impregnating such a liquid, when supplied with heated air, with germs collected from the atmosphere, bacteria, vibriones, and mucors, &c., were soon developed; the albumenoid and fatty matters, the essential oils, and pigments belonging to these organisms being derived from the elements of the ammonia salt, the phosphates, and the sugar. These complete organisms were built up out of the material afforded by such a mixture of simple substances, a fact which is quite contrary to Pouchet's declaration that ovules or germs were evolved from a sort of vitality remaining in lifeless, or, rather, dead, matter—that is to say, matter deprived of life.

A solution consisting of—

Pure water	100 parts
Sugar-candy	10 ,,
Tartrate of ammonia . .	0·2 to 0·5 part
Yeast ashes	0·1 part
Pure calcium carbonate .	3 to 5 parts

showed much the same phenomenon, in fact, differing only by a more marked tendency towards the changes called lactic, viscous, and butyric fermentations; and all ferments, whether vegetable or animal, characteristic of these changes were produced, simultaneously or successively.

Prof. Tyndall, in 1870, gave us a means of investigation, supplementary to the microscope, and of extreme delicacy. Aided by Prof. Huxley, he proved that particles in a liquid, quite invisible under an object-glass readily showing bodies $\frac{1}{100,000}$ of an inch in diameter were revealed with the greatest ease by means of a beam of light. If the air were pure, a beam of sunlight travelling a darkened room would be invisible except where it struck upon the wall. It is the scattering of the light by the floating dust which makes the track luminous, the larger and more numerous the particles the greater the luminosity. Hydrogen, coal-gas, air passed through cotton-wool, and the air of still places, were found to be free from floating matter. The writer, who has devoted much attention to this subject since 1865, made use of this discovery to aid him in a very careful repetition of some experiments published by Dr. Bastian in "Nature" of June

30th, 1870. The following few lines are a slight sketch of the results; for particulars the reader must be referred to the "Proceedings of the Royal Society" for 1872, p. 140.

Tubes cleansed with the greatest possible care, and afterwards heated to redness, were filled with solutions of the same composition as those which it was said by Bastian gave rise to organisms *in vacuo* after heating to so high a temperature as 150° C.; the water and liquids were tested according to Prof. Tyndall's method with a beam of light. After keeping for twelve months, during which time, on frequent examination with a ray of light, no change was seen to have taken place, drops of the liquids were allowed to run on to slips of glass placed in a bell-jar of hydrogen, such being a space shown to be free from floating matter. The microscope, with a higher power than that employed by Dr. Bastian, showed the solutions to be free from all organisms; nevertheless, portions let out into previously heated flasks, in a few days invariably became charged with living things. The original tubes, to which only pure air had been admitted, were kept weeks and weeks, and still no signs of life were visible in them; some of these tubes are in existence now, and still in the same condition. Here, then, were liquids, first, kept *in vacuo*, secondly, in pure air, thirdly, in ordinary air, and only under the last condition did they become filled with life, and that happened in every case. Without wishing to reflect on the work of anyone, it is simply stating a matter of fact to say, that results in favour of the theory of evolution *de novo* may be obtained most easily, and the more careless the experimenter the more successful would he be in that direction. We therefore see not only the extreme caution with which statements advancing heterogenesis should be received, but also the overbalancing weight of evidence contained in well-determined facts tending in an opposite direction.

VII. THE DOLMEN MOUNDS AND AMORPHOLITHIC MONUMENTS OF BRITTANY.

THE AMORPHOLITHIC LINES AND AVENUES.

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PART III.

MR. LUKIS deprecates the proneness of the native archæologists to dogmatise upon the intended uses and destination of these remains without a sufficient knowledge of their construction (from what we have already quoted, it will be seen that our theorists are not far behind the Breton *savants* in wild and ingenious supposition); and he partly agrees with Mr. Stuart, of Edinburgh, as to circles of stones not being temples, but sepulchral enclosures, but considers that as yet there is but insufficient evidence to show that the terminating circles of Menec and Kerlescant were used as burial-places, although Mr. Lukis himself found, in 1869, fragments of coarse clay vessels, flint scrapers, and chippings, within the area of the latter circle. Mr. Lukis comes to the following conclusion:—"*It is possible, therefore, that groups of pillars arranged in lines and circles, and associated together, may have served a purpose in some way connected with the funeral rites or solemnities that preceded interment.*" Since the above was written, Mr. Lukis has measured and planned a circle at Keswick; within this circle, and touching it, is an internal structure, which has every appearance of having served as a sepulchre; it may or may not be coeval with the circle, but Mr. Lukis's own impression is that it belongs to the original plan, and, if so, tends to confirm Mr. Stuart's view that these circles are sepulchral. It is a well-attested fact that many of the "Motes" and "Things" in Scotland were surrounded with circles of monoliths, sometimes termed "raises." That many of the circles and lines in Scotland are connected with sepulchral remains appears evident from Sir Henry Dryden's account of the following lines and circles—

"Lines, Battle Moss, Yarhouse.
Lines and cist, Garry Whin.
Lines, 'Many Stones,' Clyth.
Lines, Camster.
Circle ? Achanloch.
Circle, Guidebest, Latheronwheel.

"The groups of lines in France (of far larger stones and greater length than those in Caithness) have the largest stones, and widest intervals and the highest ground (the heads), to the W., or thereabouts, and the smallest stones, and narrowest intervals and lowest ground (the tails), to the E., or thereabouts. The Caithness groups differ entirely in principle. The one at Yarhouse loch runs N. and S., does not radiate, and is on nearly level ground; but the three others have the narrower intervals and higher ground to the N. (which end we may call the head), and radiate towards the S. and lower ground. The group at Battlemoss, near Yarhouse, is on ground falling slightly to N.W. It consists of eight lines placed N. and S. The width at the S. end is 44 ft. The lines are somewhat irregular, and appear to radiate slightly towards the N., but this is uncertain. One line extends 384 ft., and another one 170 ft., but the remaining six now only extend 133 ft. The ground is covered with peat and heather, and other stones may be hidden below the surface. There is no cairn or other grave now visible in proximity to the lines. The largest stones are about 2 ft. 6 in. high, 2 ft. 6 in. wide, and 1 ft. 3 in. thick.

"The group at Garrywhin consists of six lines. The whole width at the head (N.E. end) is 50 ft., and at the bottom 107 ft. The central lines bears N.N.E. or S.S.W. The length of this line is 200 ft. The fall is 20 ft. to the S.S.W. At the head is a cist of slabs, 3 ft. 6 in. by 2 ft. 6 in., and 2 ft. 4 in. deep, placed E. and W. As this grave is on the highest point of the knoll, and as the lines commence at it, it is fair to presume that they are connected. In the cist were found ashes, pieces of pottery, and flint chips, but no bones. As the cist is between the third and fourth lines, it is fair to presume that there never were more than six lines.

"The group called 'Many Stones' has the head on the top of a knoll from which the ground falls on all sides. The lines are on the S. slope, and are twenty-two in number. The width at the head or N. end is 118 ft., and at the bottom is 188 ft. The length in the centre is 145 ft., but there is no proof that this was the original length, and the presumption is the reverse. The average bearing is N. and S., and the fall 10 ft. 3 in. The largest stones now remaining are about 3 ft. high, 3 ft. wide, and 1 ft. 6 in. thick. There are numerous blocks of stone lying about the head, where, however, the rock is exposed, but the example of Garrywhin makes it probable that a cairn once existed on this knoll. There are no traces of any *sunk* grave, but the cairn may

have contained a chamber above ground, like many in the vicinity.

"The group at Camster is on the moor, on ground falling slightly to the S.W. A considerable depth of peat overlies the rock here, and many stones are below the surface. There are now six lines ascertained. The length is 105 ft., width at the head, or N. end, 30 ft., and at the tail, or lower end, 53 ft. The average bearing is N. and S. The stones are smaller than at the last-mentioned group. There is no cairn or other grave apparent close to these lines, but in a direction due N., at 346 ft., is a cairn. No stones are now traceable between; but as there are gaps in the lines themselves, this blank interval may once have had lines on it to connect the cairn with the existing group. No habitation *now* exists near the spot, but there were many in this strath, which may account for destruction of stones in former times. A few hundred feet farther N. is the huge horned cairn described by Mr. Anderson, and at 436 ft. N.N.E. from the small cairn is the round chambered-cairn described in the same paper."

Mr. Barnwell writes as follows:—

"In North Wales is a remarkable example of a circle and avenue, unnoticed by Pennant and other writers. The description of it is given by Miss Davies, of Penmaen Dovey, the daughter and representative of one of the most accomplished scholars and judicious antiquaries of Wales. It is situated between two streams, called Cwym-y-Rhewi and Avon-y-Disgynfa, looking down from a considerable elevation on the Vale of Mochnant, and two miles above the well-known waterfall of Pistill-y-Rhaiadr. It consists of a large circle of isolated stones, of which thirteen were remaining when Miss Davies last saw it, and an avenue of two rows still retaining thirty-nine, and many portions of others that had been broken up. In the centre of the circle is a deep hollow, the site, no doubt, of the sepulchral chamber. The name Rhos-y-beddau, or *the graves on the moor*, has rescued the monument from being claimed by the Druids. The avenue appears to lead directly into the circle, the breadth of it corresponding to the space between the two stones of the circle where the circle and avenue meet, but it is probable that a stone or two is wanting at this part of the circle.

"In the northern part of Pembrokeshire is a single line of stones of great size, which Fenton does not mention, although he deliberately pulled to pieces a fine cromlech near it, and which seems to have been connected with this row of stones,

for it was probably continued further northwards than it is at present. On referring to the Ordnance Map, a little to the right of the word 'Llanlawer,' will be seen the position of the line called in the map 'Parc-y-marw' (field of the dead); and a little further to the east, but slightly to the north, is marked down the cromlech destroyed by Fenton, and of which only some small fragments remain. The line of stones is parallel to the narrow road, and if continued would pass within a few paces of the ruined cromlech. Here, as at Rhos-y-beddau, the name points to the character of the monument; for experience has shown that local names of this kind in Wales, handed down from time immemorial, may be generally depended on. Local tradition, however, adds an account of a desperate battle fought on the spot, among the pillar-stones themselves, as if the possession of them were said to have been the sole object of the combatants. A lady, clad all in white, appears to those who are rash enough to walk that way by night; and so ancient is this tradition, which is still firmly believed, that a short distance before the stones commence, a foot-path, by long use now become public, turns across the fields to the left, making a *détour* of nearly a mile before it leads again into the road. During day-time the peasants do not think it necessary to take the roundabout course. The road itself is evidently one of great antiquity, and apparently led to the great work at Dinas. The height of the stones is not so striking, as their lower part is embedded in the tall bank of earth that does the duty of an ordinary edge; but some of them are full 16 feet long."

Mr. Lukis having shown conclusively that the lines of Carnac constitute not one monument, but three distinct groups, proceeds to compare them with Avebury. He remarks that now there is very little clue to its original plan, and that we are compelled to accept the inaccurate drawings of antiquaries of the seventeenth and early part of the eighteenth centuries. Whilst he confesses himself sceptical with regard to the ground-plan of Avebury as given by Stukeley, his doubt is strengthened by his intimate acquaintance with the Carnac and other groups of stone lines in Brittany. He prefers the more careful drawing in the plans of Aubray to the fanciful restoration of Stukeley, and gives as his opinion that the remains at Avebury were originally three distinct monuments; viz., one group of concentric circles, and short avenue, on Overton Hill; the second, of the larger circles and avenue of Avebury; whilst the third monument of like character, *i.e.*, composed of rows of stones

associated with a circle, lay on the Beckhampton side. Mr. Lukis, however, feels that he has very little evidence in support of his views, with which, however, he will find many archæologists ready to agree. Beyond the fact that in both the Avebury and Carnac remains circles are associated with avenues, he finds the points of resemblance few and faint, and the points of dissimilarity numerous and strong: however, as one point of resemblance, he states that in Brittany the circular enclosure is invariably situated on an elevation, or on the summit of gently rising ground. In Wiltshire, one set of concentric circles is on Overton Hill, and the great circle of Avebury is also on a gentle elevation. Thus far, although the comparison of Avebury has not done much towards the elucidation of Carnac, yet the example of Carnac has taught us to look at Avebury in a new light.

Among the points of dissimilarity are the following, viz.:—At Carnac there are many—ten, eleven, and even thirteen rows of stones; at Avebury there were never more than two. With the Brittany circles there is no vallum or fosse, nor are there any concentric circles, all of which features appear to be characteristic of the Wiltshire remains.

Sir Gardner Wilkinson describes the stone lines of Dartmoor as leading up to concentric circles with cromlechs or kists, and as therefore being in some way connected with sepulchral and religious rites. Again, Mr. Spence Bate, in his supplementary report on the prehistoric remains of Dartmoor, mentions an extensive avenue in the neighbourhood of Corydon Ball, consisting of seven or eight rows, extending at least a hundred yards, with suggestive traces of what may have formed portions of a circle at the eastern extremity. A huge cairn, with a portion of a kist, are also mentioned near the same locality. It would be interesting to compare the seven or eight rows of stones at Corydon Ball with those described in this paper as to their parallelism or convergence, &c.

There are systems of avenues of stones with circles in various other parts of the world—in Lombardy, Africa, India, &c. We may quote the elaborately ornamented megalithic avenues leading to the tombs of the emperors of China as modern developments of the primæval structures. Thus we read that the great tomb (the Ling or resting-place of Yung-Lo, of the Ming dynasty), thirty miles from Pekin, consists of an enormous mound or earth-barrow, covered with trees. Its height is not mentioned, but it is evidently considerable, from the fact that the circular wall which surrounds it is a mile in circumference. In the

centre of the mound is a stone chamber containing the sarcophagus in which is the corpse. This chamber or vault is approached by an arched tunnel, the entrance to which is bricked up. This entrance is approached by a paved causeway, passing through numerous arches, gateways, courts, and halls of sacrifice, and through a long *avenue* of colossal marble figures, sixteen pairs of wolves, kelins, horses, camels, elephants, and twelve pairs of warriors, priests, and civil officers. Whether this avenue is orientated or not is not noticed, but an idea of the size of these colossal marble figures may be formed from the following:—"During the building of the late Emperor Heen-fung's tomb, a road one hundred miles long was made from the quarries of Fangshan to the Tung-ling, and a block of marble fifteen feet long, twelve feet high, and twelve feet broad, weighing sixty tons, was seen by several of us then resident in Pekin, being dragged along this road on a strong truck or car drawn by six hundred mules and horses." . . . . "This block was to be cut into the figure of an elephant to be placed as one of the guardians of the tomb."—(W. Lockhart, Proc. R. G. S., 1866).

Similarly, near Nankin, there exist avenues of colossal stone figures, attributed to the same Ming dynasty, in connection with the tombs, but what these tombs consist of is not mentioned. More south, in Fokhien, and doubtless throughout southern China, are found the horse-shoe or omega-shaped tombs which in some cases are associated with analogous approaches. Although not covered by artificial tumuli, the sepulchral chambers are excavated in the side of the natural hills, whilst those belonging to high officials are approached through avenues of stone pillars and carved figures, animal and human, although on a much smaller scale than those of Pekin and Nankin. A sketch of a group of these tombs, said to be those of former governors of Canton, at the foot of the White Cloud mountains, is exhibited.

Now we may venture to assume that all cromlechs, dolmens, kists, and other sepulchral stone chambers of every description, were originally covered with tumuli. Some of the tumuli appear to have had their bases strengthened by revetments or boundary walls of large upright stones. In Great Britain and the Channel Islands we frequently find that the tumuli have disappeared, leaving the structures thoroughly denuded of the smaller stones, earth, or sand which originally covered them, whilst the large blocks forming the revetment remain, and have been generally

termed "*peristaliths*." These features certainly are unusual in Brittany, where, however, there are some examples,—at Kerlescant, Plouneour, and elsewhere. Now lately the author ventured to suggest that the circles of stone in Brittany and elsewhere might be looked upon as the possible remains of colossal "*peristaliths*," the sole indications of gigantic tumuli which may formerly have filled their interior space, and which have now disappeared by atmospherical, aqueous, and human agencies during the lapse of centuries. Nor need we much wonder if no trace of the actual sepulchral chambers within be left, when we consider that the largest tumuli have generally been found to contain the most insignificant kists; besides, it is far from improbable that the builders of the huge mounds, such as those at Mont St. Michel, &c., in the immediate neighbourhood of the lines and circles, constructed their barrows from the material afforded by the *débris* of the more ancient tumuli within the circles.

Mr. Fergusson, in his recent work on "Rude Stone Monuments," gives John Stuart ("Sculptured Stones of Scotland") the credit of having first remarked—"Remove the cairn from New Grange and the pillars would form another Callernish;" but thirty-seven years ago Mr. Leshingham Smith * notices the ingenious suggestion of the Messrs. Anderson, viz., that "*the circles usually called Druidical temples are nothing more than cairns without the loose stones*."

Since, however, the above suggestion was offered by the present writer to the late Ethnological Society, he (the author) is altogether inclined to admit the conclusion to which Fergusson has arrived, viz., that the stone circles in Europe appear to have been *introduced in supercession* to the circular earthern mounds which surround the early tumuli of our downs. These earthern enclosures still continued to be used surrounding stone monuments of the latest ages, but, if Mr. Fergusson is not mistaken, also gave rise to the form itself. For instance, the circle at Stanton Moor—called the nine maidens—may be looked upon as a transitorial example.

The circular mound, which is thirty feet in diameter, enclosed a sepulchral tumulus, as was no doubt the case from time immemorial, but in this instance was further adorned

* *Vide* "Excursions through the Highlands and Isles of Scotland in 1835 and 1836," by the Rev. C. Leshingham Smith, M.A., Christ's College, Cambridge.

and dignified by the circle of stones erected upon it. A century or so afterwards, when stone had become more recognised as a building material, the circular mound may have been disused, and then the stone circle would alone remain. Fergusson also figures a woodcut, taken from Haxthausen's work of the uncovered base of a kurgan or tumulus at Nikolajew, in the government of Cherson, which he suggests may give us a hint as to the genesis of circles. The tumulus was cleared away, and its base was found to be composed of three or four concentric circles of upright stones surrounding what appears to be a kist of five stones in the centre. Similar arrangements have been found in Algerian tumuli, and it looks as if the first kist of the sepulchral circle may have arisen from such an arrangement having become familiar before being covered over, just as Fergusson supposes the free-standing dolmen to have arisen from the uncovered cist having excited such admiration as to make its framers unwilling to hide it. In fact, just as the free-standing dolmen and cromlech may be looked upon as the skeletons of original chambered tumuli after the flesh of the sepulchral mound, which gave meaning to the structure, had disappeared, so we may look upon the circle as the representative of the revetting peristalith which formerly encircled the tumulus, but which tumulus was ultimately never filled in; and similarly, we shall not be far wrong in looking upon the avenues which lead to circles as a development of the funnel-shaped narrow entrances to these same chambered tumuli. That they were intended for permanence is evident, and the people who erected them must have had similar associations of ideas regarding life and death as had both the Egyptians and Buddhists; the former, according to Diodorus Siculus, called the dwellings of the living mere "*lodging-houses;*" their tombs, on the contrary, they looked forward to as their "*eternal homes.*"

Anyhow, whether there were actually tumuli or not within these circular enclosures, the sepulchral theory seems the most fitting conclusion to arrive at; and if this be so, then the avenues may be looked upon as approaches of a ceremonial character connected with funeral rites, not necessarily only those which *preceded* interment, but for *subsequent* visitations, as shown by the permanent construction of these monuments, which were evidently intended to last through future ages.

As to this day in China the clans and families annually revisit the tombs of their ancestors for the purpose of worship and sacrifice, repairing and cleaning the graves, and

placing food for the dead, &c., so through the alignments of Brittany may have passed at stated periods of time to do honour to the resting-place of their forefathers, the descendants of those whose bones rested within the sepulchral circles.

That there is some connection, as regards the funeral rites practised from ancient times, by the most orthodox of the Chinese, with *oriculation* and *stone pillars* appears plain from the following, taken from "The Life and Teachings of Confucius," by J. Legge. "According to the statutes of Hea, the corpse was dressed and coffined at the top of the eastern steps, treating the dead as if he were still the host. Under the Yin the ceremony was performed between the *two pillars*, as if the dead were both host and guest. The rule of Chow is to perform it at the top of the *western* steps, treating the dead as if he were a guest."

That the custom of surrounding the sepulchres of mighty kings is of remote origin throughout the East is evident from what we know of the funeral ceremonies practised at the time of the invasion of Western Asia by the Scythians, 625 B.C. Thus the *Chakravartins*, a branch of the great Scythian race, or Sakas, were styled the Wheel-kings—in fact, *Kings of the Circle—i.e.*, monarchs who ruled all within the *chakra* of rocks supposed to surround the world. Hence, as the symbol of universal authority, the tombs of these kings, after their cremation and certain recognised ceremonies, were surrounded by a circular range of rocks or unhewn stones—in fact, *amorpholiths*, to signify that they were Lords of the Universe. So Sakya Buddha requested that he should be buried according to the rules of the Chakravartins, *i.e.*, that his remains—after undergoing certain prescribed ceremonies—should be burned, and his tomb erected in the method known among the Sakas or Sakyas, viz., by raising over his ashes a vast mound of earth, and surrounding it with the usual emblems of authority—the circle of amorpholiths. How fully this rule was attended to in the erection of topes or stûpas is too well known to need illustration. These topes or stûpas were at first only mounds of earth, included within a circular wooden rail or ring of steles, as we find in India and Ceylon. But when the munificence of Asoka was brought to bear on the subject, these old and barbarous mounds were destroyed, and topes faced with stone—in many instances magnificently wrought and ornamented—came into date. But in these the original idea was never lost sight of; they are all designed to indicate the authority of a universal monarch—

not a monarch only of the world, but according to the expanded creed of Buddhism at the time of Asoka, lord of the "*three worlds*," also :—(1). The world of men, signified by the square plinth on which the dagoba rests surrounded by the circular rail. (2). The world of Dêvas, signified by the dome or vault of heaven; and (3). The world of space, signified by the kchêtra that rises from the Tce, ending in the symbol of the boundless empyrean—the three-forked flame or trisul. (Catena of Buddhist Scriptures, by S. Beal).

In Ceylon, the bell-shaped reli-shrines or Dagobas are surrounded with concentric circles of monoliths of various numbers.

Thus, at Thuparamya (250 B.C.), there are three concentric circles; and at another, on the hill of Mehentele, the concentric rows of granite pillars rise to half the height of the central mound. At Sandei and Amravati also we find the well known circles; at the former in the shape of stone imitation of wooden railings, and at the latter in two concentric circles of upright stones (193 feet outer diameter) carved with minuteness.

In India stone worship is very prevalent, and, in consequence, the custom of erecting amorpholiths is not yet extinct. In every part of Southern India four or five stones may often be seen in the ryots fields, placed in a row and daubed with red paint, which they consider as guardians of the field, and call the five Pandus. Col. Forbes Leslie supposes that this red paint is intended to represent blood. The god of each Khond village is represented by three stones. Col. Leslie gives the drawing of a group of sacred stones near Delgaum, in the Dekkan: the three largest stood in front of the centre of two straight lines, each of which consisted of thirteen stones. These lines were close together, and the edges of the stones were placed as near to each other as it was possible to do with slabs which, although selected, had never been artificially shaped. The stone in the centre of each line was nearly as high as the highest of the three that stood in front, but the others gradually decreased in size from the centre, until those at the ends were less than a foot above the ground into which they were all secured. Three stones, not fixed, were placed in front of the centres of the group. All the stones had been selected of an angular shape, with somewhat of an obelisk form in general appearance. The central group and double lines faced nearly east, and on that side were whitewashed: on the white, near, although not reaching quite to, the apex of

each stone, was a large spot of red paint, two-thirds of which from the centre were blackened over. Dr. Hooker, too, remarks that among the Khasias "funeral ceremonies are the only ones of any importance, and they are often conducted with barbaric pomp and expense; and rude stones of gigantic proportions are erected as monuments, singly or in rows, or supporting one another like those of Stonehenge, which they rival in proportions." Major Godwin Austen describes some trilithons of the Khasias of immense size. The great stone of one of these monuments weighed 23 tons 18 cwt., and another is described as measuring 30 ft. by 13 ft. and 1 ft. 4 in. in thickness, and supported on massive monoliths. Mr. W. F. Holland also describes circles of massive stones as existing in the Peninsula of Sinai.

Mr. Fergusson has well shown how in India the tumulus has developed into the tope, and the tope into the temple. It is almost to be wondered at that he did not notice the extraordinary analogy between the groups, rows, and avenues of unhewn stones, and those thousand-pillared *chaöries* and *choultries* of the Southern Hindu temple-builders whose most important application is their use as *nuptial halls*, in which the annual mysteries sacred to the union of the male and female divinities are celebrated. Their other uses are, according to Fergusson, in his "Handbook of Architecture," most various—serving as porches to temples, as halls of ceremony, cloisters—where the dancing girls dance and sing—or as swinging-porches for the gods, who appear to have been pleased with such innocent amusement. At Tinnevelly, for instance, the great pillared hall has 100 columns in its length by ten in width, so that it would have 1000 pillars, were not twenty-four omitted to make way for a small temple.

At Chillumbrum, the hall is twenty-four pillars wide by forty-one in length, which, adding the sixteen of the porch, would make up the number; but some are omitted in the centre, to make space for ceremonies, so that the actual number is only 930.

At Seringham the hall is of about the same extent, and several other temples have halls, the number of whose pillars varies from 600 to 1000. In most instances no two pillars are exactly alike.

The temple of Tiruvalur measures externally 945 ft. by 701 ft. In the outer court, and towards the principal entrance, is the great *choultry*, intended apparently to have had 1000 columns, being sixteen pillars wide by forty-three in depth, *one half, however, of them support no roof*, so that the

structure, according to Fergusson, is "*probably*" (or, as he says in another place, *evidently*) unfinished. If this great temple is really finished, as does not seem improbable, we have here some hundreds of carved pillars forming an approach of several avenues, which have been erected within recent times for a specific object.

Fergusson, it must be borne it mind, gives no reason for supposing that this edifice is unfinished, or that the architects ever contemplated putting a roof on these columns, and it is certainly well worth noticing, and enquiry should be made upon this subject.

The whole number of columns standing is 688; they are all equally spaced, except that there is a broad aisle down the centre, and a narrower transverse avenue in the direction of the entrance. Fergusson gives a plate (No. 65), taken from Ram-Raz's "Hindu Architecture," which shows the forest of pillars supporting no roof. Fergusson likens the great choultry to the Stoa Basilica of Herod's restored Temple.

Another analogy in the great development of stone avenues may be noticed in the avenues of sphinxes extending for miles on the banks of the Nile, connecting the Hypostyle Halls, Pylons, &c., of the palace-temples of great Thebes.

Fergusson's conclusion as to the age and destination of the Carnac stone rows may be summed up briefly as follows:—

(1.) That it is most improbable that a temple should extend over six or seven miles of country; in fact, he hardly knows any proposition that appears to him so manifestly absurd as that these stone-rows were temples, and he feels sure that no one who thinks twice of the matter will venture again to affirm it.

(2.) It seems equally clear that they were not erected for any civic or civil purpose. No meetings could be held, and no administrative functions could be carried on in or around them.

(3.) They are not sepulchral, in the ordinary sense of the term, as nowhere were men buried in rows like this, extending over miles of heath and barren country; moreover, the French *savants* have dug repeatedly, and found no trace of burial. "It no doubt is true that the long barrow at Kerlescant, the dolmen at Kermario, and the enclosure at Menec, may have been, *indeed, most probably were, burying-places*, but they can no more be considered the monument than the drums and fifes can be considered the regiment. They are only adjuncts; the great rows must be considered as essentially the monuments." (Why so?)

(4.) Being neither temples nor town-halls, nor even sepulchres, they must be trophies—the memorial of some great battle or battles.

So far as to Mr. Fergusson's conclusion as to their interpretation—next, as to their date :—

(1.) Cæsar never mentioned them, therefore they could not have existed when he wrote his Commentaries.

(2.) No mediæval rhapsodist ever attempted to give them a pre-Roman origin.

(3.) The event represented by these stone-rows therefore is limited to the period which elapsed between the overthrow of the Roman power by Maximus, A.D. 383, and the time when the people of the country were converted to Christianity in the early part of the sixth century.

(4.) Finally, Grallon was engaged in two wars—one against the Romans, and the other against the Norse pirates—and it is to this, as connecting the stone monuments with a northern people, that Fergusson is inclined to ascribe the erection of the Carnac alignments. In fact, they commemorate a battle or campaign fought between the years 380 and 550 A.D., the Arthurian age.

It may be safely left to our readers to decide whether they are satisfied with this decision, after perusal of the foregoing notes; but we cannot conclude without observing that if the Veneti erected the lines of amorpholiths, whether they were temples, sepulchres, trophies, or town-halls, they would have certainly handed down to their present descendants, the modern Morbihannais, their true character and meaning, which at present is as much an obscure enigma to them as it is to all who have yet enquired into this subject. It is to be hoped that a more satisfactory conclusion than that of Mr. Fergusson's may yet be arrived at.

NOTICES OF BOOKS.

Our Seamen: an Appeal. By SAM. PLIMSOLL, M.P. London: Virtue and Co.

SURELY a more terrible book than this has never been written. It differs from all other narratives of the terrible. In all fearful natural catastrophes the remembrance that what has happened has been inevitable has its influence in fortifying the mind. In reading of destructive wars or battles, we recognise some object which in the view at least of the combatants has rendered the destruction of life and property a necessary evil. Narratives of plague and pestilence are generally adorned by acts of heroism which cause us almost to forget the horrors of the events with which the narrative deals. Shipwrecks, in like manner—only not such shipwrecks as the book before us deals with—have their grand episodes. And, moreover, in war and battle, plague, pestilence, and famine, in shipwreck and explosion, we seldom have instances of the deliberate destruction of human beings by their fellows. Nay, even such events as the Massacre of St. Bartholomew or the Reign of Terror in the French Revolution, have usually resulted rather from the inversion of a high motive than from any utterly base and sordid consideration.

But in the book before us we have the account of the systematic destruction of life and property for certain sums of money. We see bands of men sent to almost certain death by a contrivance as terrible as the boat of Nero, but calculated to operate on a far larger scale. And more marvellous than all, we see bodies of men, ready for the sake of a moderate wage, to face what amounts very nearly to the certainty of death; though by an ingenious arrangement of our laws matters are so arranged that a part of this heroism commonly depends on the dread of the disgrace of imprisonment in our common gaols.

At the root of the system leading (if all that this book says can be maintained) to these fearful results, is the system of insurance employed as against sea-risks. This system is probably but little known to the general public. We propose to give a brief account of its peculiarities. In the first place, a ship is not insured by any one Company, but by a large number of persons, who (from the mode in which the risk is accepted) are called "underwriters." Each of these accepts a very small part of the risk. Accordingly, if a ship is lost, and there are reasons to fear that there has not been fair play, each underwriter has but a small interest in making any inquiry into the affair. But this is not all. No underwriter is strong enough to dispute a claim. An underwriter so acting incurs odious misrepresentation and suspicion, and, as a rule, by one such act completely ends his career as an underwriter,—this too, even though "the brokers

through whom future business is to come are fully satisfied that he did right, that the disputed claim was founded in fraud."

Is it necessary to point to the consequences of such a system? The great bulk of our shipowners are, doubtless, altogether free from suspicion. But in any large body of men, there will always be some few who are ready to gain money by any means available to them. The system of underwriting offers such means. A ship may be bought which is unseaworthy, or may be sailed until repairs are absolutely essential to her safety, or may be built without the necessary precautions to ensure her from breaking up under blows which a stouter ship would resist. Such a ship may be overloaded until from this cause alone she is unsafe. And every voyage she makes thus overloaded repays the owner better than a safe journey with a moderate load. But then she may be insured for more than the value of ship and cargo; and her destruction may be rendered practically a certainty by overloading her until she could only sail safely with the lightest breeze. She may even be overloaded to such an extent as to ensure her destruction within sight of the port she is leaving. "A large ship put out to sea one day," says Mr. Plimsoll in the book before us. "She was so deep that T. M. said to me as she went, 'She is nothing but a coffin for the fellows on board of her.' He watched and watched, fascinated almost by the deadly peril of the crew; and he did not watch for nothing. Before he left his look-out to go home, he saw her go down."

It might be supposed that the men capable of thus trading on the lives of men and on the present system of insuring ships would soon be recognised and avoided by the underwriters. But, unfortunately, a long time is required to establish a character as a completely unscrupulous insurer. "In the meantime, ship after ship goes down, and with them the lives of sailors mostly in the prime of manhood. In a northern port some years ago, there was a collier fleet well known by the name of 'X's coffins.' When these shipowners fail to find regular insurance, they still have the resource of joining mutual security clubs; and even without this they often find it *pays* to go on sending out very old and infirm ships, which would bring nothing if offered for sale." "Ships are insured as long as possible, and when re-christening and all other dodges fail, even with underwriters, then they form mutual insurance clubs, and go on until the ships fill and go down in some breeze, or strike and go to pieces."

It is singular that Mr. Plimsoll, who notices everything else which would strengthen his case with the commercial public, fails to notice how shipowners must needs suffer by this system. We may be sure the underwriters do not suffer in the long run, or they would give up insuring. What happens, then? Why, manifestly, sea-risks are increased, and the honest shipowners have to pay higher rates to cover the increase of risk due to the dishonest insurers. It is thus the interest of the shipowners as

a body (for as a body they are just men) to remove the evil from their midst. And this we may safely say, that no shipowner with a grain of sense, and whose conscience is clear of offence, can oppose Mr. Plimsoll's plea for a full inquiry into these horrors.

Mr. Plimsoll considers that a law against over insuring, and another requiring that ships unfit for the sea should not be allowed to sail, are the main requirements to meet the occasion. The cases he cites in support of this view should be read and studied by all who wish to understand how the matter really stands. His book is full of interest apart from the great object which he has in view; and as we are all more or less interested in the welfare of our commercial marine, the present treatise should be, and we trust will be, widely read. No one who reads it will refuse Mr. Plimsoll the heartiest wishes for his success; and we believe that most of his readers will give him real assistance in his efforts to remove a great scandal from our midst.

The Eruption of Vesuvius in 1872. By Prof. LUIGI PALMIERI, of the University of Naples, Director of the Vesuvian Observatory. With Notes and an Introductory Sketch of the Present State of Knowledge of Terrestrial Vulcanicity, by ROBERT MALLET, Mem. Inst. C.E., F.R.S, &c. London: Asher and Co.

THE publishers of this work have done well in securing the services of Mr. Mallet to introduce Prof. Palmieri's "Incendio Vesuviano" to the English public. Mr. Mallet's mastery of the subject of seismology and vulcanology is unsurpassed; and we owe to him the definite enunciation of what will be before long accepted—we entertain little question—as the true theory of terrestrial vulcanicity. It was obviously desirable that the description of so important a seismological event as the recent eruption of Vesuvius should be submitted to the investigation of one who would not regard it in its sensational aspect, or merely in its historical relation to former events of the kind, but would recognise its true scientific aspect. It is, however, to be noted that Prof. Palmieri himself is a true student of science. Mallet justly remarks, Palmieri's "Narrative of the events of the eruption is characterised by exactness of observation, and a sobriety of language, so widely different from the exaggerated style of sensational writing that is found in almost all such accounts, that I do the author no more than justice in thus expressing my view of its merits."

The volume before us is about equally divided between Mr. Mallet's introduction and Prof. Palmieri's account of the late eruption. We shall consider the two portions separately, since, as a matter of fact, they are distinct in subject matter.

In the introduction, Mr. Mallet sketches what appears to him to be the present position of terrestrial *vulcanicity*, tracing the

outlines and relations of the two branches of scientific investigation—*vulcanology* and *seismology*—by which its true nature and part in the cosmos are chiefly to be ascertained." He remarks, by way of defining his subject, that "Vulcanicity properly comprehends all that we see or know of actions taking place upon and modifying the surface of our globe, which are referable not to forces of origin above the surface, and acting superficially, but to causes that have been or are in operation beneath it. It embraces all that Humboldt has somewhat vaguely called "the reactions of the interior of a planet upon its exterior." He indicates the relation between astronomy and physical geology, which overlap each other, through vulcanicity. He then sketches the history and progress of knowledge in the chief domains of vulcanicity. In discussing the more recent contributions to the science, commencing with his early paper "On the Dynamics of Earthquakes," which appeared early in 1846, he takes occasion to point out that Mr. Hopkins, of Cambridge, in his Report "On the Geological Theories of Elevation and Earthquakes," read before the British Association in June, 1847, did him some injustice. He remarks, that if his paper be compared with Mr. Hopkins's Report, it will be found that as respects the earthquake part, the latter work parades in a mathematical dress some portion of the general theory of earthquake movements, previously published by Mr. Mallet. "This," he proceeds, "is but too mystifyingly suggestive of the 'Pereant qui mea ante mihi dixerunt'" (a somewhat novel rendering of the hackneyed quotation, by the way). We dwell on this point, because in Prof. Phillips's "Vesuvius," the injustice (unintentionally, of course) is continued, and the theory of earthquakes is too important a contribution to science to be handed over to one who certainly was not its author. The definition of an earthquake in Mr. Mallet's paper of 1846 sufficiently indicates the main teaching of his theory; an earthquake he there says, is "The transit of a wave or waves of elastic compression in any direction, from vertically upwards or horizontally, in any azimuth, through the crust and surface of the earth, from any centre of impulse or from more than one, and which may be attended with sound and tidal waves dependent upon the impulse and upon circumstances of position as to sea and land." The whole paper should, however, be carefully studied by those who wish to form a just opinion of the position in which Mr. Mallet stands with respect to the view of earthquakes, soon to become the established theory on the subject.

From the date of the publication of that paper until that of the paper recently contributed by him to the Royal Society, Mr. Mallet has continued his researches, experimental and mathematical, and the views to which he has been led may be regarded as affording, in the main, the most complete and satisfactory account of the phenomena of earthquakes and volcanoes yet

extant. Passing over those portions of his views which relate to the period when our globe first liquefied from the nebulous condition, and to the earliest stages of cooling by radiation, when the crust was extremely thin, as also his account of the deformation of the spheroid as one of the first effects of its contraction, we find that he has endeavoured to show that the rate of contraction of the crust while very thin exceeded that of the large fluid nucleus supporting it, and so gave rise to *tangential tensions* in the crust, fracturing it into segments; but next, "that as the crust thickened, these *tensions* were gradually converted into *tangential pressures*, the contraction of the nucleus now beginning to exceed (for equal losses of heat) that of the crust through which it cooled. At this stage these tangential pressures gave rise to the chief elevations of mountain chains,—not by liquid matter by any process being injected from beneath vertically, but by such pressures mutually reacting along certain lines, being resolved into the vertical, and forcing upwards more or less of the crust itself. The great outlines of the mountain ranges and the greater elevation of the land were designated and formed during the long periods that elapsed in which the continually increasing thickness of the crust remained such that it was still, as a whole, flexible enough, or opposed sufficiently little resistance to crushing to admit of the uprise of mountain chains by resolved tangential pressures." "As our earth is still a cooling body, and the crust, however, now thicker and more rigid, is still incapable of sustaining the tangential pressures to which it is now exposed, so it is by no means inferred that (relatively) slow and small movements of elevation and depression may not still and now be going on upon the earth's surface; in fact, all the phenomena of elevation and depression, rending, &c., which at a much remoter period acted upon a much grander and more effective scale." "But the thickness of the earth's crust, thus constantly added to, by accretion of solidifying matter from the still liquid or pasty nucleus, as the whole mass has cooled, has now assumed such a thickness as to be able to offer a too considerable resistance to the tangential pressures to admit of its giving way to any large extent by revolution upwards; yet the cooling of the whole mass is going on, and contraction though unequal, both of thick crust and of hotter nucleus beneath also, whether the latter be *now* liquid or not." "For equal decrements of heat, or by the cooling in equal times, the hotter nucleus contracts more than does its envelope of solid matter. The result is now, as at all periods since the signs changed of the tangential forces, thus brought into play, *i.e.*, since they became tangential pressures; that the nucleus tends to shrink away, as it were, from beneath the crust, and to leave the latter, unsupported or but partially supported, as a spheroidal dome above it." Mr. Mallet shows that, in this state of things, and under the actual conditions to which the crust of the earth

is subjected, this crust must *crush*, "to follow down after the shrinking nucleus. . . . . It must crush unequally, both regarded superficially and as to depth; and the crushing will not be absolutely constant and uniform anywhere or at any time, or at any of those places of weakness to which it will be principally confined, but will be more or less irregular, quasi-periodic, or paroxysmal; as is, indeed, the way in which all known material substances (more or less rigid) give way to a slow but constantly increasing steady pressure."

Such is a brief sketch of the general views of Mallet; but for the details, and particularly for the estimates of the rate at which the vulcanic processes now in progress are taking place, and an account of the experiments conducted to obtain these estimates, the reader is referred to the present work.

It is hardly necessary to point out how much the interest of Palmieri's narrative is enhanced by its association in this treatise with Mr. Mallet's inquiries into the phenomena of the earth's crust. In fact, Mr. Mallet has specially tested his views by a study of the phenomena presented during the last two thousand years by Vesuvius, "the best known volcano in the world."

Nevertheless, it is to be noted that Palmieri's Memoir contains much which does not bear directly on Vulcanology. It will be none the less interesting on this score, however, to the general reader; and we recommend all those who are desirous to learn all the circumstances of a great and characteristic eruption of Vesuvius, to turn to the pages of this book. As Mr. Mallet well remarks, "a special narration such as this should not suffer in popular estimation by the fact that Prof. J. Phillips has so recently given to the world the best general account of Vesuvius in its historical and some of its scientific aspects which has yet appeared."

Papers relating to the Transit of Venus in 1874. Prepared under the Direction of the Commission Authorised by Congress. Published by Authority of the Hon. Secretary of the Navy. Part I. Washington.

THESE papers consist of a series of letters on the subject, the most important being those from Mr. Rutherford, and of an essay "On the Application of Photography to the Observation of the Transits of Venus," by Professor Newcomb. Mr. Rutherford describes his method for photographing the sun as a guide to the method of photographing the phenomena of the transit; and then says—"If the whole matter of ordering instruments for the photographing of the transit of Venus were in my control, with my present lights, I should have an achromatic objective of five inches aperture, and seventy inches focus, in a cell which would allow of the application, in front of it, of a lens

of flint-glass of such curves as would shorten the focal distance (for photographing) to sixty inches. At the proper point, I would place between the two distances an enlarging-lens so constructed that the normal image of the sun in the principal focus (then about half an inch) would be enlarged to two inches at the distance of ten inches from the principal focus, viz., at 70 inches from the objective. The camera-box and tube should be one tube, and the focalising rack and screw should be located at the objective end of the tube, thus simplifying the whole arrangement and permitting the use of braces, from end to end, to prevent flexure; and on taking off the photographic corrector, and taking out the enlarging-lens, the instrument will be all ready for vision. On consideration, I do not think I would counsel a smaller telescope than the one I have named."

Professor Newcomb divides the proposed methods of observing the transits of Venus into two classes. The first consists in fixing the moment at which the planet is in contact with the limb of the sun; the second, in determining the relative position of the centre of the planet and the centre of the sun as often as possible during the transit. The first method, although only that has hitherto been thought practicable, Prof. Newcomb conceives liable to inaccuracies; and he proposes photography as the aid to the second method, in order to form an image of the sun with Venus on its disc, so that points on the plates corresponding to the centres of the discs can be fixed with precision, the linear distance between these points being determined by means of a micrometer, and the angle of position obtained from a reference line—this line bearing a relation to the circle of right ascension passing through the sun's centre. For the details of the process of photographing the transit, the corrections necessary in the glasses, we must refer the reader to the original papers.

There are some considerable difficulties connected with this method. The greatest difficulty would appear to be, that the required element appears only as a minute difference between two comparatively long arcs, too long to be measured by a micrometer. But Professor Winlock's apparatus would remove many of the disadvantages.

These papers contain so much important matter that we hope soon to see the second part.

Memoirs of the Geological Survey of England and Wales. Vol. iv.: *The Geology of the London Basin.* Part I.: *The Chalk and the Eocene Beds of the Southern and Western Tracts.* By WILLIAM WHITAKER, B.A. (Lond.). (Parts by H. W. BRISTOW, F.R.S., and T. Mc. K. HUGHES, M.A.) London: Longmans, Green, and Co.; and Stanford. 1872.

IT is obviously a matter of convenience to the public that the Maps of the Geological Survey, as they are issued sheet by

sheet, should be accompanied by short explanatory memoirs. But, since it necessarily happens that the areas comprised within these sheets are bounded in an arbitrary fashion, it becomes in the highest degree desirable that—as the work of the Survey progresses, and districts with well-defined natural limits are worked out—the information scattered through the shorter memoirs on the separate sheets should from time to time be gathered together and expanded into special volumes, each devoted to a full description of the geology of some extensive tract of country, bounded by well-marked physical features. Such a volume is the admirable memoir by Mr. Whitaker on the Geology of the London Basin.

Much misconception prevails respecting the true nature of this so-called "Basin." Misled by the popular use of the term, and accustomed to the caricatured sections given in most geological works, one finds it difficult to realise the very gentle nature of the trough in which the metropolis is seated, and the true dip and relation of the beds within the London area. But on studying the sections issued by the Geological Survey, which are drawn on the same scale horizontally and vertically, it is immediately seen that the disturbances which have affected the strata in the south of England have been of the tamest possible kind—that there have been no vast foldings of the beds, no great elevation here or depression there—and that such high-sounding phrases as "the great arch of the Weald," or "the deep trough of the London Basin," are equally deceptive. "When compared with its horizontal extent," says Mr. Whitaker, "the vertical displacement in the latter area is indeed trifling."

The chalk is the lowest formation exposed within the London Basin, though well-sections have reached the Upper Greensand, the Gault, and certain lower beds—perhaps of Neocomian age, or even older. Above the chalk come the Lower Eocenes, comprising the Thanet beds, the Woolwich and Reading beds, the Oldhaven beds, and the London Clay. It may not be amiss to remark the name "Oldhaven beds" was proposed by Mr. Whitaker, in 1866, for some sands and pebble-beds equally distinct from the London clay above and from the Woolwich beds below. These beds are well exposed at Oldhaven Gap, on the Kentish coast, near the Reculvers. Passing from the Lower to the Middle Eocenes, we find in the London Basin the Lower, Middle, and Upper Bagshot series; but the beds above these are not represented in the London area, and to study the Eocenes it is necessary to cross to the Hampshire basin. As to the various superficial deposits, they are well enough exposed, it is true, within the London basin, but it formed no part of Mr. Whitaker's plan to describe them, as it is proposed that they shall form the subject of the second part of this volume.

In the systematic preparation of this memoir, Mr. Whitaker's

course has been to notice the several geological formations in ascending order—first describing their general nature, and then detailing their range, their lithological characters, and the sections in which they are exposed.

Having thus described the nature and range of the various formations, the author devotes one chapter to the disturbances which the beds have suffered, and another to the physical features which they present. In discussing the causes which have given to the country its present contours, Mr. Whitaker clearly shows that the surface has been sculptured into its present form of hill and scarp and dale by subaërial denudation, rather than by marine action—that, in fact, the varied features of the scenery are mainly, if not exclusively, due to meteoric agencies—to rain, rivers, frost, and the like—agencies which are ever silently at work under our eyes, and are fully competent to effect all that has been ascribed to their action, if only sufficient time be granted for the work.

The concluding chapter of the Memoir is devoted to Economic Geology—a subject which, in this area, does not admit of very extensive treatment. But perhaps the most valuable part of the work—as a work of reference—is to be found in its copious Appendices. One of these, on the Bibliography of the subject, shows in a remarkable manner Mr. Whitaker's extensive acquaintance with geological literature; whilst the second Appendix contains details of upwards of 500 well-sections and borings within the area under description. Finally, Mr. Etheridge and some other palæontologists contribute valuable lists of fossils from the beds of the London Basin.

Before closing the work we should remark that, though the great bulk of the text has been written by Mr. Whitaker, certain parts have been contributed by his colleagues—Mr. H. W. Bristow, F.R.S., Director of the Geological Survey of England and Wales; and Mr. T. McK. Hughes, M.A., the new Woodwardian Professor of Geology at Cambridge.

The publication of this elaborate volume leaves no longer any excuse for ignorance on the geology of the country around London. It must, however, be confessed that the physical features of the country within a moderate distance of the metropolis are not such as tend to foster geological tastes; and, in spite of the labours of Mr. Prestwich and of the Geological Survey, we fear that among the millions who dwell within the area of the London basin, there are comparatively few who know anything of the true nature of the ground beneath them. "Turpe est in patriâ vivere, et patriam non cognoscere."

The Theory of Strains in Girders and Similar Structures, &c. By BINDON B. STONEY, M.A. London: Longmans, Green, and Co. 1873.

THE constantly growing demand for education, in every path in life, must soon have the effect of replacing those hard-headed, practical, self-taught, but untheoretical engineers who, it cannot be denied, have been the pioneers of the profession, and to whom credit is due for the construction of many important and magnificent works. But, as is stated by Mr. Stoney in his preface, "practice which was formerly excusable, or even worthy of the highest commendation, would, now that knowledge has increased, be properly described as culpable waste." At the present day, the engineer requires not only to be a practical man, but he should also be well acquainted with the physical laws by which his works are regulated, so that he may at once combine strength and refinement in his structural details.

Nothing can be more important, in connection with engineering structures, than a complete knowledge of the strength of the materials employed, and this again requires to be augmented with a full appreciation of the duty to be performed by each portion of such structures; in other words, of the strain or stress to which each such portion is subjected, and of its capabilities to resist it. The work now before us is a handbook to such knowledge, so far as iron structures are concerned, accompanied by observations on the application of theory to practice, and tables of the strength and other properties of materials, compiled from such authorities as Hodgkinson, Tredgold, Wertheim, Young, Fairbairn, Barlow, &c.

The principle of strains is based on the fact that on the application of force all bodies change either form or volume, or both together. For convenience sake such strains are divided under five heads, namely, tensile, compressive, transverse, shearing, and torsional strains, according as they are caused by tearing asunder, crushing, breaking across, cutting, and twisting asunder, respectively. As the strength of any material depends ultimately on its capability of sustaining strains, it is of essential importance to know the ultimate resistance to tension or compression which each material possesses, and thence deduce those strains which may be safely imposed in practice; and the object of the present work is to put before the student in this branch of science the results of the investigations, carefully worked out, by those who have given more particular attention to the subject. Besides the strains of tension and compression, the elongation and shortening of the material subject to strain claims attention, for experience has proved that the safe working strain of any material does not exceed its sensible limit of uniform elastic reaction, generally called the limit of elasticity. This limit may also be defined to be the greatest strain that does not produce an appreciable set.

The investigation of transverse strains may be reduced to the three following fundamental laws in mechanics, viz., the resolution of forces, the law of the lever, and the equality of moments, upon which are founded all the investigations given of the strength of materials when subject to transverse strain.

After an introductory chapter, our author enters upon a consideration of the circumstances of flanged girders with braced or thin continuous webs, when subjected to six different conditions of weight or strain. In this part of the work the formulæ investigated refer to transverse strains only, the horizontal strains in braced or thin continuous webs being so inconsiderable that they may be practically neglected. All girders have what is called a *neutral surface* and a *neutral axis*, the former being that surface along which the resultant of the horizontal components of all the diagonal forces equal cipher, and the latter the line of demarcation between the horizontal elastic forces of tension and compression exerted by the fibres in that particular section of the girder. The sum of the moments of the horizontal elastic forces in any transverse section round any point whatsoever is the *moment of resistance* of that particular section, or, as it is also called, the *moment of rupture*. The coefficient of rupture varies, of course, with different materials, and, in order to enable the formulæ given on the above subject to be the better applied, a table of coefficients is given.

As the result of several investigations, it is laid down as a rule that the strength of similar girders varies as the square of their lineal dimensions, but the weight of the girder itself varies as the cube of its lineal dimensions.

Space will not admit of our following Mr. Stoney's book in such detail as the interest of the subject would otherwise justify. The calculations given respecting one class of girders are continued to girders of various sections, to girders with parallel flanges but having webs formed of various-shaped bracings, to girders with oblique or curved flanges, &c. The chapter on "Deflection of Girders" is an important one, showing, as it does, that girders of uniform section throughout are often defective from a scientific point of view. In all properly constructed girders each part is duly proportioned to the maximum strain which can pass through it, so that no material is wasted; and when this occurs in a girder with horizontal flanges and a uniformly distributed load, that is, the load which produces the maximum strain in the flanges, these latter will taper from the centre, where their section is greatest, towards the ends as the ordinates of a parabola.

Having considered and given rules for the quantity of materials, and the angle of economy for braced girders, there follow chapters on torsion, the crushing strength of materials, and rules for the strength of pillars, whether circular or braced. These, of course, have reference to the construction of piers and abut-

ments, or other supports for girders, and are most important. The tensile strength of materials is very fully discussed, and is followed by chapters on *Shearing-Strains*, *Elasticity and Set*, and *Temperature*. Next follow chapters on the detail parts of girders and bridges, such as *flanges*, *web*, *cross-girders and platform*, *working load*, &c.; after which several pages are given to *estimation of girder work*, which forms a most fitting sequel to what has preceded. The book concludes with an appendix, in which many interesting and detailed particulars are given of the Boyne Lattice Bridge, on the Dublin and Belfast Junction Railway, which affords a practical illustration of the theories laid down in the main body of the book.

On the Cause, Date, and Duration of the Last Glacial Epoch of Geology, and the Probable Antiquity of Man. With an Investigation and Description of a New Movement of the Earth. By Lieut.-Col. DRAYSON, R.A., F.R.A.S., &c. London: Chapman and Hall. 1873.

IN spite of all that has been written—whether by geologist, astronomer, or physicist—in explanation of the different conditions of climate in past phases of the earth's history, the subject still remains so enshrouded in obscurity that light from any quarter should be gladly greeted. Perhaps the most remarkable—certainly the most interesting—of these climatic conditions is represented by that period which geologists recognise as the Glacial Epoch,—an epoch in which arctic conditions seem to have prevailed over the northern hemisphere down to at least the forty-fifth parallel of latitude. It is universally conceded that this episode in the history of our planet occurred in comparatively recent geological times, but we are as ignorant of its absolute date and period of duration as of the physical causes by which it was brought about. It is, however, to the solution of these problems that Col. Drayson addresses himself in the present work.

Rather than offer his own description of the Glacial Epoch, the author cites copiously from the writings of Ramsay, Lyell, Agassiz, Page, and other geologists. He then discusses and dismisses the several theories which have from time to time been advanced with the view of explaining the cause of these glacial conditions,—such as the passages of the earth, with the rest of the solar system, through zones of space of different temperatures; the assumed changes in the excentricity of the earth's orbit; differences in the distribution of the great masses of land and water; and alteration in the position of the earth's poles by shifting of the axis. It is strange that we fail to find here any reference to the writings of Mr. Croll, who has, of late years, so ably discussed some of these theories.

In introducing his own explanation, Col. Drayson begins by examining the three principal movements of the earth—its rotation on its axis, its revolution round the sun, and especially the slow movement of its axis round the pole of the ecliptic. It is almost universally laid down by astronomical authorities that the pole of the heavens moves in a circle round the pole of the ecliptic, as a centre, constantly maintaining an angular distance of 23° 28′ from that centre. The author seeks to refute this generally-accepted proposition, and endeavours to prove that the earth's axis describes a circle—not round the pole of the ecliptic as a centre, but round another centre 6° distant from the pole. As the full astronomical discussion of this movement is reserved for a forthcoming volume, we withhold criticism on this part of the work, and confine ourselves to the geological consequences which tend to flow from the author's data.

Assuming Col. Drayson's premisses, it follows that during one revolution of the pole of the heavens round the pole of the ecliptic, occupying about 31,840 years, there must be a variation of 12° in the obliquity of the ecliptic. This variation is sufficient to account for extraordinary changes of climate on the surface of the earth. It is calculated that at the date 13,702 B.C. the obliquity was at its maximum, namely, 35° 25′ 47″. At that time, therefore, the arctic circle would be brought down to this distance from the pole, and our own islands would consequently come within the frigid zone. But whilst our winters were thus characterised by arctic severity, the author argues that the summers must have been almost tropical. In winter, then, the country would be covered with a complete mantle of ice, and in summer this would be rapidly thawed, giving rise to heavy floods and vast numbers of icebergs.

We have seen that, according to our author, the Glacial Epoch was at its height in 13,700 B.C. He believes, however, that the occurrence of great alternations of temperature, producing marked effects on the climate, extended over a period of about 16,000 years—8000 before and 8000 after the maximum. The glacial period would, therefore, have begun in 21,700 B.C., and terminated in 5700 B.C.

Assuming the course of the pole to be uniform, there would be a recurrence of glacial periods every 31,000 years. Prof. Ramsay, from the study of certain beds of breccia, long ago insisted on the necessity of recognising earlier glacial periods; and the very phrasing of Col. Drayson's title, "The *Last* Glacial Epoch," shows that he, too, believes in previous periods of a like character. Few geologists will, however, agree with the author in his curious suggestion that these extreme climatic conditions may account for the alternation of different beds in our coal-measures, much less for the bands of flint in our chalk.

In closing Col. Drayson's work, the geological reader, though anxious to accept many of his conclusions, will feel that he must

be guided mainly by the verdict of the astronomer. After twelve years of patient thought upon his favourite subject, the author ventures to maintain that certain time-honoured principles in Astronomy require correction. That he is thus bold enough to be original is no reason why his propositions should not be candidly discussed. Every new idea makes its way in the world with difficulty; and we hope that the mere novelty of the author's views, whether right or wrong, will not preclude him from a fair hearing by men of science. "The imputation of novelty," says Locke "is a terrible charge amongst those who judge of men's heads as they do of their perukes—by the fashion; and can allow none to be right but received doctrines."

The School Manual of Geology. By J. BEETE JUKES, M.A., F.R.S., &c. Second Edition, revised and enlarged. Edited by ALFRED J. JUKES-BROWNE, of St. John's Coll., Cambridge. Edinburgh: Adam and Charles Black. 1873.

WITH the exception of the classic writings of Sir Charles Lyell, there are perhaps no modern text-books better known to the student than the Manuals of the late Prof. Jukes. His were no mere compilations, as elementary treatises too often are, but were the work of a field geologist, whose heart was in his hammer. The success of Jukes's larger volume, the "Students' Manual," induced the author, about ten years ago, to write an introductory work, under the title of the "School Manual of Geology." Since the lamented death of Prof. Jukes new editions of both works have been called for; the editing of the larger Manual was entrusted to the author's colleague, Prof. Geikie; that of the smaller Manual to the author's nephew, Mr. A. Jukes-Browne.

Since the original appearance of the "School Manual" geology has made great advances. But whilst duty to the reader has compelled the editor to effect many alterations, he has wisely forborne, from respect to his uncle's memory, to unnecessarily interfere with the original plan of the work. Jukes's "School Manual" remains, then, what it has always been—one of our best text-books for the student of elementary geology.

Geological Stories; A Series of Autobiographies in Chronological Order. By J. E. TAYLOR, F.G.S., &c. London: Hardwicke. 1873.

JUDGING from the large number of "Play-Books of Science" constantly being issued, there must be a large section of the reading public desirous of acquiring a smattering of scientific knowledge without the labour of systematic study. To such readers Mr. Taylor's "Stories" will be peculiarly acceptable.

Written in a pleasing gossiping style, they lead the reader smoothly onwards, until he finds himself in possession of a great deal of geological information.

These stories have, for the most part, already appeared in "Science Gossip," a journal of which the author is editor; but they are now arranged in chronological order, so as to present a simple and picturesque view of the past history of our Earth. The autobiographies are told by pieces of granite, quartz, slate, limestone, sandstone, coal, rock-salt, jet, Purbeck marble, chalk, clay, lignite, the "Crags," a boulder, and a gravel-pit.

Whilst recommending these "Stories" to the class of readers for whom they were primarily intended, we cannot help remarking an unsatisfactory looseness of expression, common to most popular writings, but annoying to the scientific reader. For example, confining ourselves to the first chapter,—the story of a piece of granite,—we object to alumina, soda, potash, lime, and other oxides, being constantly called "elements;" nor are we pleased to hear the chemical constituents of mica and of felspar spoken of as "mixed" together in these minerals respectively. But the most curious statement in this chapter is that felspar may be detected in a mass of granite by being "so soft that you may scratch it with your finger-nail!" If this extraordinary assertion is made on the authority of personal examination, it is clear that either certain parts of the author's exo-skeleton had acquired an unwonted degree of induration, or the specimen under test was advancing to a state of kaolinisation. Mr. Taylor is evidently more at home when speaking of fossils than of minerals; and, as might be supposed, we find him at his best in the later chapters, from the "Story of the Crags" onwards.

Despite any little defects observable here and there, the work contains an attractive collection of stories well calculated to quicken a taste for geology in those who may be too careless about the grand Science of the Earth to apply themselves to the study of systematic treatises.

The Owen's College Junior Course of Practical Chemistry. By H. E. Roscoe, B.A., F.R.S., Professor of Chemistry in Owen's College, Manchester, and Francis Jones, Chemical Master in the Grammar School, Manchester. London: Macmillan and Co.

The number of elementary works on chemistry which have been lately issued from the English press proves that the importance of this science is at last beginning to receive something like due recognition. Amongst these treatises few are likely to prove more valuable than the one before us, which bears the impress of having been arranged by one who, like Prof. Roscoe, has

learnt from prolonged experience what the student exactly needs. The synoptical tables for the detection and qualitative separation of the elementary bodies, when occurring in compounds or mixtures, are well arranged. The student who can give a correct reply to the questions contained in the last section will have laid a firm foundation, and will be well prepared for turning his attention to the higher departments of the science. We can therefore confidently recommend this Manual, both to students and to teachers of chemistry, as an excellent syllabus for a practical course of instruction.

The General Glaciation of Iar-Connaught and its Neighbourhood, in the Counties of Galway and Mayo. By G. H. Kinahan, M.R.I.A., Of the Irish Branch of the Geological Survey of the United Kingdom; and M. H. Close, M.R.I.A. Dublin: Hedges, Foster, and Co. 1872.

The mapping of both kinds of the glacial phenomena considered in this pamphlet was commenced seven years ago by Mr. G. H. Kinahan, and was carried on during the course of his work on the Geological Survey. The pamphlet may be said to include a complete view of the glaciation of the district, although many admirable notices and descriptions have appeared from the pen of Prof. King, Messrs. Ormsby and Campbell, and others.

Signal Service U.S. Army: Telegrams and Reports for the Benefit of Commerce and Agriculture. Published by Order of the Secretary of War.

"They do these things better abroad," is as true of meteorology as of many other instances of perhaps more personal moment. A vast number of observations have been shown by Mr. Norman Lockyer to be necessary to the determination of a weather-cycle, and it may be considered probable that the nation first achieving a collection of these data will be the first to make a decisive step in meteorological science. If this hold goods, America certainly appears the country to which the honour will accrue. We have received a copy of these telegrams published during one day by the U.S. Army Signal Service, accompanied by two weather maps. The telegrams give (for seventy-three stations) the height of the barometer, the change in the last eight hours, the temperature and change in the last twenty-four hours, relative humidity, the direction, velocity, and pressure (in the per square foot) of the wind, the amount and direction of upper and lower clouds, the rain-fall, and the state of weather at each station.

These particulars are issued and telegraphed thrice daily, and during the day there are also issued maps of the Continent, showing what has been the state of the weather during the last four-and-twenty hours, and what will probably be the state of the weather during the next twenty-four. Upon the immense importance of such numerous details it is impossible to be too emphatic; a similarly perfect system should be demanded by science from our own Government before it should be too late to reap the full benefit of the labours of English meteorologists. We know that great progress has been made in our own meteorological department, but still we are very far from the advanced ground of our American cousins, who will quickly bear off the palm in this respect, if it be not already gone.

PROGRESS IN SCIENCE.

MINING.

CONSIDERING the present exceptional state of the coal-market, it is by no means surprising that public attention should eagerly fasten on the news of any discovery of coal, or of kindred mineral, which may perchance afford a seasonable supply of fuel. Unusual currency has, therefore, been given to certain announcements respecting the recent discovery of coal in different parts of the British Isles; but most of these announcements refer to localities where the existence of mineral fuel has long been known to the geologist, and we must confess that at present there seems no likelihood of any really new centres of coal-mining being established.

Whitecliff Bay, at the eastern extremity of the Isle of Wight, is one of these coal-bearing localities, to which attention has recently been directed. It appears that the gales in the Channel have swept away much of the sand and shingle which usually cover the shore of the bay, and have laid bare what has been described as a seam of coal, from 6 to 7 feet in width, extending in a straight line from the foot of the cliffs down to low-water mark. The tertiary strata in Whitecliff Bay rest in an almost vertical position against the highly-inclined chalk, and, striking directly across the island from east to west, reappear on the opposite side, in the well-known section in Alum Bay. Now it happens that in Alum Bay beds of lignite have long been known to occur in that division of the Middle Bagshot series known as the Bracklesham beds. Prof. Ramsay and Mr. Bristow examined these beds in 1860, and observed that each seam was based upon a stratum of clay resembling the underclay of the coal-measures. Some few years ago similar beds were detected by the Geological Survey in Whitecliff Bay; and the recent discovery resolves itself into a fresh exposure of these Bracklesham coals. It is not, however, likely that these seams will ever prove of any commercial value.

Rumours are afloat of great discoveries of coal and cannel in Sutherlandshire. Yet, as far as can be gathered from authentic sources, it seems that these reputed discoveries refer merely to the coals and shales of the Middle Oolites of Brora, well known to every geologist. No one denies that in certain parts of the world great deposits of Mesozoic coal are extensively and profitably worked; but, bearing in mind the very limited occurrence of such coals in our own islands, it seems doubtful whether their commercial development will ever be remunerative to the British capitalist.

In Elginshire there are not wanting voices to advocate a search for coal, in spite of the adverse geological conditions of the locality. It is true Mr. Judd's admirable researches on Scottish Geology have recently placed beyond doubt the fact that the celebrated Elgin sandstone must be referred to the New Red and not to the Old Red sandstone—a conclusion to which Prof. Huxley's study of its reptilian fossils had previously pointed. Nevertheless this conclusion does not in any way lend itself to the support of those views on the probability of finding coal which have found expression in some of the Scotch papers. For it is surely the height of geological folly to suppose that every bit of New Red must needs have coal-measures beneath it: and, indeed, the highest geological authorities are of opinion that these measures were never deposited within the area of the Elgin sandstone. We are glad to observe that the "Athenæum" has called attention to the folly of this projected enterprise, which has been so staunchly, yet unscientifically, supported by the "Elgin Courant."

Some valuable researches on the conditions under which safety-lamps are truly safe have been conducted by Mr. R. Galloway, partly at the new Laboratories at South Kensington, and partly at the Meteorological Office. Mr. Galloway has already found that if a Davy lamp be burning tranquilly in

an explosive atmosphere, the transmission of a sound-wave, produced by a slight explosion of gunpowder, is sufficient to determine the communication of flame from the lamp to the surrounding atmosphere. Hitherto it has been generally assumed that the occurrence of a colliery explosion, after firing a shot, is due to actual communication of flame from the gunpowder to the fire-damp; but Mr. Galloway's experiments show that it is much more likely that the explosion is determined by the noise of the shot being propagated through the galleries of the mine to the safety-lamps. An admirable experiment to illustrate this point was exhibited by Dr. W. Spottiswoode at a recent lecture at the Royal Institution. A lighted Davy lamp was surrounded by streams of coal-gas issuing from a number of jets round the base of the lamp. One extremity of a long tin tube, open at both ends, was placed in connection with the lamp, while a pistol was fired at the other end, a caoutchouc diaphragm being interposed in the tube to prevent the transmission of a direct current of air. The sound-wave, generated by the report, travelled along the tube, and, as soon as it reached the flame, caused ignition of the surrounding atmosphere—the lamp being immediately enveloped in flames.

An improved self-extinguishing safety-lamp, which appears to combine security, simplicity, and strength, has been patented by Mr. Yates, of Duke Street, Westminster. The lower part of the lamp, containing the reservoir of oil, is furnished with a locking-bolt, which bears against some ratchet teeth fixed to the base of the upper portion, or cage of wire-gauze. While the lamp is being screwed into its cage the bolt runs readily over these teeth, but when the two parts are screwed together it is impossible to unscrew them until this bolt has been withdrawn. This is effected by turning a milled head attached to the unlocking screw at the base of the lamp, but the very act of turning this screw causes the wick to be so depressed in its socket that before the lamp can be opened the flame is effectually extinguished. It therefore becomes impossible for the miner to tamper with his lamp without immediate extinction of the light. Nor is there any inducement to open the lamp for lack of light, for unusual brilliancy is obtained by a silver-plated concave reflector fixed behind the light, and a strong well-annealed glass lens secured in a metal frame in front. It is further claimed for the Yates lamp that, though giving a brilliant light, it consumes much less oil than is usually burnt in the ordinary Davy lamp.

A description of the remarkable deposits of Fossiliferous Iron Ore in Southern Pennsylvania, by Prof. B. Silliman, has been published in the "Journal of the Iron and Steel Institute." These deposits occur in a group of Silurian rocks, known locally as the "Surgent Shales"—the equivalent of our Wenlock beds. Three zones of ore are found on three distinct horizons in these shales—the Levant ore, the Twin beds of fossil ore, and the Hæmatites at the top of the series. By far the most important of these iron ores are the Twin beds of fossil ore,—an ore notable for its purity, its uniformity of structure, and its wide distribution. "It is believed," says Prof. Silliman, "to be, of all deposits of iron ore in the known world, the most extensive and important."

Attention has been called, by Prof. Hull, the Director of the Geological Survey of Ireland, to the brown hæmatite occurring in the Lower Silurian rocks, at several localities in the counties of Longford and Cavan.

A description of the iron ores of Nova Scotia, and the manufacture of Acadian iron, has appeared in the "Mining Journal." It seems likely that the present high price of iron may lead to the development of the iron-producing resources of this colony.

METALLURGY.

Some improvements in effecting the removal of phosphorus from pig-iron, during the process of puddling, have been introduced by Prof. Scheerer, of the Mining Academy of Freiberg, in Saxony. Chloride of calcium and chloride of sodium are mixed in about equal proportions, and the two salts fused

together. The fused mixture is run into waterproof paper cases, each holding about 2 lbs., and these cases are introduced into the puddling-furnace one by one, so that the dephosphorising mixture may be thoroughly incorporated with the charge. To ensure satisfactory results, it is recommended to use three times as much of the mixture as the iron contains phosphorus.

According to a method of preparing steel lately proposed by Messrs. F. Bajault and M. Roche, a mixture of cast-iron and powdered hæmatite is smelted, and the product cast in the form of pigs, these pigs being then heated for a considerable time in a furnace of peculiar construction. The rough steel thus obtained may be melted, either in crucibles or in a reverberatory furnace. A sample of steel prepared in this manner yielded, on analysis—Combined carbon, 0·43 per cent; uncombined carbon, 0·8; silicon, 0·13; sulphur and phosphorus, none.

Mr. H. Defty, of Middlesbro'-on-Tees, has patented his trunk-refinery and puddling-furnace. This furnace is provided with an inclined revolving chamber, surrounded at intervals by cast-iron clips which bear upon pulleys on an inclined shaft, rotated by steam-power. The molten iron passes from the smelting-furnace into the chamber, and the lining of this chamber assists in bringing the metal into a malleable state. The metal passes into an oven at the lower end of the chamber, where it is received, with the slag, in a bogie or ingot-mould; whilst the products of combustion, passing from the furnace through the chamber, are utilised in the cupola furnace employed in preparing the metal.

A new system of fettling with oxide of iron, recommended for use in the manufacture of finished iron, has been patented by Mr. T. Greener and Mr. W. Ellis. The mill-furnace in which such fettling is used should have a gradual fall from the fore-plate of from $\frac{1}{2}$ to $\frac{3}{4}$ of a foot; the refuse from the iron gradually collects in its descent towards the flue, and is there tapped into a bogie. Before use in the puddling-furnace the fettling is broken up in a Blake's crusher.

Although the manufacture of charcoal-iron is not at present carried on to any great extent in France, there are still a few furnaces which treat high-class ores, and produce a charcoal-iron of first quality. As such works usually possess sufficient hydraulic power to keep the machinery in motion, and as the hot-blast is but rarely employed, the only means of utilising the waste gases from these blast-furnaces seems to be their employment in the puddling-furnace or in the refinery. A method of using these gases has been patented by M. de Langdale, and has been described by M. V. de Lespinats in the "Bulletin de la Société de l'Industrie Minérale." The gases are taken off by a common English cup-and-cone, and are then washed and cooled by a shower of water, so that the aqueous vapour present is effectually condensed. The necessary temperature is obtained in the puddling-furnace by using Siemens's regenerators.

Dud Dudley's quaint treatise, entitled *Mettalum Martis*, has been reprinted, by request, in the "Journal of the Iron and Steel Institute" (1872, vol. ii., No. 4). The same number of this journal contains a translation, by Mr. Ernest Bell, of a German paper, "On the Working of Blast-Furnaces with Raw Coal, at Gleiwitz, in Upper Silesia," by Dr. Wedding. There will also be found in this journal an abridged translation of the Report of the Commission appointed by the iron-masters of Belgium to visit this country and examine the working of Danks's rotary puddling-furnace, at Middlesborough.

A new technical journal is devoting itself to the interests of the iron trade. The old-established "Mechanics' Magazine" has arisen in an entirely new shape, and, under the title of "*Iron*," now forms a useful weekly journal dedicated to metallurgy and allied branches of industry.

MINERALOGY.

Considerable interest was excited a short time back by M. Jeremejew's announcement that he had discovered diamonds imbedded in a rare Russian mineral known as Xanthophyllite.* Wishing to verify Jeremejew's observations, Dr. Knop, of Carlsruhe, has been quietly working at the subject, and has recently come to the conclusion that the so-called crystals of diamond are merely angular cavities, suggesting, it is true, the well-known forms in which the diamond is wont to crystallise, but nevertheless destitute of the veriest trace of diamond, or of any other mineral substance. It might, however, be fairly supposed that the cavities, though now empty, originally contained certain crystalline materials which impressed their angular form upon these hollows. Some curious experiments by Knop lead, however, to an opposite conclusion. He obtained thin sections of xanthophyllite, which, when magnified 1500 diameters, appeared to be absolutely destitute of any of these angular cavities: nevertheless, after treating the preparation with sulphuric acid, numerous cavities were recognised exactly similar to those referred in other cases to the presence of diamonds. In other experiments, fine lamellæ of xanthophyllite were carefully examined in all directions under the microscope, and the entire absence of any crystalline impressions thus determined; the object was then touched with a few drops of concentrated sulphuric acid, and heated until white fumes appeared. The preparation, when cooled, was protected with a cover-glass, and placed under the microscope, when it exhibited swarms of beautiful tetrahedral cavities, sharply defined, regularly formed, and arranged in parallel rows. From these and other observations, the author feels justified in concluding that the angular cavities in the Russian xanthophyllite have nothing to do with the presence of diamonds, but owe their origin merely to the corrosive action of acids.

Further "Mineralogical Notices," by Prof. Maskelyne and Dr. Flight, have been published in the "Journal of the Chemical Society." The first portion of the present communication refers to the heterogeneous substances grouped together under the name of *Isopyre.* It appears that these are, for the most part, merely impure forms of opal, associated with other mineral substances. The rare species to which Brooke, many years ago, gave the name of *Percylite*,—an oxychloride of lead and copper, occurring in beautiful blue crystals, belonging to the cubic system,—has hitherto been known only by a single specimen, said to have come from Sonora, in Mexico, and now exhibited in the British Museum. It is, therefore, of interest to learn from Prof. Maskelyne that Percylite has been found among minerals from South Africa. Among other points of interest in this paper, a comparison is suggested between certain minerals from Redruth, in Cornwall, and those recently discovered at Schneeberg, in Saxony, and attention is called to the simultaneous presence of bismuth and uranium in association with arsenic, in minerals from these widely-distant localities.

Another recent contribution to British mineralogy is due to Prof. Church, who has communicated to the Chemical Society some analyses of certain mineral arseniates and phosphates. The minerals examined comprise some transparent crystals of the fluor-apatite known as *asparagus stone* (Werner's *Spargelstein*); the rare species *arseniosiderite*, which occurs in a deposit of manganese-ore at Romanêche, Macon; and the West of England minerals—*Childrenite*, *ehlite*, *tyrolite*, and *wavellite.*

Breithaupt, the venerable mineralogist, though retired from the professorship which he so long held in the Mining Academy of Freiberg, in Saxony, has not rested from his labours. Unable, through loss of sight, either to read or to write, he dictates to his assistant, Herr Frenzel, and has thus been able to contribute to Leonhard and Geinitz's "Jahrbuch" some recent "Mineralogical Notices." Among these notices he gives the full characters of the mineral which he described some time ago as *Nantokite.* This is a chloride of

* See Quart. Journ. Science, No. XXXII., Oct., 1871, p. 541.

copper from Chili, containing Cu_2Cl_2, which on exposure to the atmosphere readily becomes encrusted with atacamite; and it is suggested that most if not all the atacamite is probably formed from Nantokite. *Winklerite* is the name given by Breithaupt to a new Spanish mineral, of which large quantities are said to have been sold in England as cobalt-ore. From an analysis by Dr. C. Winkler the following unattractive formula has been deduced:—

$$8(5CoO.2CO_2+4H_2O)+6(Co_2O_3.H_2O)+8(2CuO.CO_2+H_2O)+$$
$$+4(2CaO.As_2O_5+6H_2O).$$

A new mineral-species has been described by Dr. Lasaulx, under the name of *Ardennite*. It is a brown or pale yellow mineral, occurring in fibrous masses without distinct crystalline form. Analysis shows it to be a silicate of alumina and manganese, with small quantities of magnesia, lime, and ferric oxide; but what is especially notable is the presence of 6·17 per cent of vanadic acid. The mineral comes from Ottrez, in the Belgian Ardennes—whence the name.

Ardennite seems to be the same mineral which M. Pisani has lately described under the name of *Dewalquite*, but his analysis gives only 1·8 per cent of vanadic acid.

The rare Scotch mineral described by Brooke, in 1820, as *Lanarkite*, has been recently studied by Pisani. According to Brooke, it is a sulphato-carbonate of lead, partially soluble with effervescence in nitric acid, and leaving a residue of sulphate of lead. Unable to observe this behaviour, Pisani has been led to analyse a typical specimen of Lanarkite from Leadhills, in Lanarkshire. He finds no carbonate of lead, though upwards of 7 per cent of carbonic anhydride is recorded in some of the older analyses. According to Pisani, the mineral known in most collections as Lanarkite is merely a basic carbonate of lead.

We owe to the same energetic mineralogist a recent analysis of the mineral termed by M. Adam *Arite*. This is an amorphous substance, resembling nickeline, and found in a vein in Mont Ar, at the foot of the Pic de Ger, in the Basses-Pyrénées. The analysis leads to the formula $Ni_2(Sb,As)$, and therefore shows that Arite is not a distinct species, but is merely an arsenical variety of the mineral long known as Breithauptite.

M. Pisani has also published an analysis of the New Jersey mineral *Jeffersonite*, from which it appears that this species belongs to the group of pyroxenes.

The third part of Dr. Carl Klein's "Mineralogische Mittheilungen" has been published, but is a purely crystallographic memoir, descriptive of the zinc-blende and anatase of the Binnenthal, in Switzerland.

It is worth recording that Dr. Kenngott has found, in a specimen of basalt from Landeck, in Silesia, some curious enclosures of quartz.

M. Daubrée has presented to the French Academy of Sciences a note, by Mr. L. Smith, describing the mass of meteoric iron which fell, in 1862, at Victoria West, Cape Colony. The iron yielded on analysis—Iron; 88·83; nickel, 10·14; cobalt, 0·53; phosphorus, 0·28; and small quantities of copper.

The same communication contains some remarks on the mineral *Enstatite*, a silicate of magnesia originally described by Prof. Shepard as *Chladnite*.

Some researches, by M. Pisani, on the native amalgams of silver occurring at Kongsberg, in Norway, show that two distinct amalgams are found,—the one containing Ag_6Hg, and therefore identical with *Arquerite*; and the other containing only 4·9 per cent of mercury, corresponding to the formula $Ag_{18}Hg$. Should the latter prove to be a definite species it is to be called *Kongsbergite*.

A paper "On the Composition of some Zeolites" has been read before the Glasgow Philosophical Society, by Mr. J. Wallace Young. The paper contains analyses of Scotch specimens of analcime, thomsonite, natrolite, and stilbite,—the alkalies in which have been determined by Lawrence Smith's method.

The little green pebbles commonly sold to tourists in Iona have been analysed by Mr. E. C. C. Stanford, and found to be a variety of serpentine, notable for containing manganese.

ENGINEERING—CIVIL AND MECHANICAL.

Guns.—The first 35-ton gun, known by the name of the "Woolwich Infant," has recently formed the subject of a report, as to the state of its interior, by the Inspector of Ordnance. After thirty-eight horizontal discharges, after its bore had been enlarged to 12 inches, the interior had sustained injuries, caused by four cracks, four fissures, and some deep roughnesses: two of the cracks were on the lower side of the bore, and all the other injuries on the upper side, their centres coinciding with the point where the front studs of the shot-hammer and the rear studs come into driving bearing. The gun is being rebuilt at a cost of about £700 or £800.

On the 9th of January last Commander Dawson, R.N., read a paper, at the Royal United Service Institution, on the "Powder-Pressures in the First 35-ton Gun," illustrated by diagrams showing the state of its interior on leaving and on returning to the gun-factories, and by corresponding diagrams of some other disabled Woolwich guns. After thirty-five discharges from the 11·6-inch bore, this gun was reduced by boring it up to 12-inch calibre, with a corresponding reduction of the pressures. But after thirty-eight horizontal discharges the 12-inch bore was so injured by the projectiles as to necessitate the rebuilding of the gun. The table of pressures shows that when the 12-inch bore was fresh from the factory, 110-lb. W. A. P. charges gave very regular maximum pressures of 20·1 tons; but when the 12-inch bore had sustained thirty-four to thirty-eight discharges the registers were very irregular, and averaged 31·3 tons. Similarly, the first 115-lb. W. A. P. charges, in the 12-inch bore, gave very regular mean maximum pressures of 22·5 tons, but subsequently increased to 44·5 tons; and the 120-lb. charges began at 20 tons and increased to 66 tons. The whole of the injuries in the bore of the gun are, however, recorded in a certain short part of the bore outside the area of maximum pressures, and precisely where the oblique movement of the axis of the projectile about its studs would have its greatest force. The same misdirection of mechanical forces was shown to be in operation in other guns similarly rifled, tending to impede the free exit of the shot, to injure the projectile and the guns, and to diminish the velocities and striking force, whilst giving rise to accumulation of gases and elevation of pressures in the bore.

Shells.—A series of experiments have recently been carried out in Austria in order to test the relative merits of steel and chilled iron shells. These trials appear to have been very exhaustive, and the results confirm what had previously been arrived at, namely, that the former are superior for naval purposes. The steel projectiles were found to pierce the shield with a considerable excess of force without breaking up, whilst the chilled shells only penetrate to the second plate and break up. The effect of the live shells also accords with this; the steel shells explode in the wood backing, and their fragments are hurled behind the target. The chilled shells burst in front of the second plate, and thus virtually produce no effect, being kept by the side far from the interior of the ship.

Dynamite.—A very interesting series of experiments have recently been carried out with this powerful explosive, among the sand-hills of Ardeer, on the coast of Ayrshire, where the British Dynamite Company have erected their factory. These experiments were carried out in order to satisfy the traffic-managers of the Scotch railways of the almost total immunity from danger that is displayed by this valuable material under all conditions of carriage. They were conducted under the superintendence of Mr. A. Nobel, the patentee and technical director of the Company. The results were most conclusive and satisfactory, proving the dynamite to be perfectly harmless under mere percussion, or when subjected to flame, but capable of exerting a most powerful effect when exploded in the usual manner with a Bickford fuse.

The mode of using dynamite is to make it into cartridges, and a percussion-cap very similar to an ordinary gun-cap is fixed to the end of the fuse. The cartridge having been opened at one end, the cap is pressed into the dynamite, and secured there by ordinary twine. When used for mining purposes, the cartridge having been placed in the bore hole, and damped with water or sand, the fuse ignites the cap, and the explosion of the cap explodes the dynamite. It has been proved by experiments that a cartridge containing 3 ounces of dynamite has as much disruptive effect as 1 lb. of powder.

Railways.—The Institution of Civil Engineers has been occupied during the whole of six or seven evenings with the discussion of a paper by Mr. W. T. Thornton, of the Public Works Department, India Office, on "The Relative Advantages of the 5 ft. 6 in. Gauge and of the Metre Gauge for the State Railways of India, particularly for those of the Punjab." The author, in his paper, after referring to estimates for narrow-gauge lines by Mr. Hawkshaw and Mr. Fowler, drew an average between the results of the two estimates, and thus attempted to prove that the saving to be effected by the introduction of narrow-gauge lines into India would not be less than £1000 per mile, which, for the 10,000 miles of State railways already in contemplation, would show a total saving of not less than ten millions sterling in their construction. And it was stated that belief in its superior economy was the one solitary reason why the Indian Government had adopted a narrow gauge for its State railways. After going into a lengthened discussion, having reference more particularly to the Punjab railways, the case for the Government of India was summed up thus:—That by making the Punjab lines on the metre gauge it would save £530,000, at the lowest computation. To have adopted a light standard, instead of a metre gauge, would have occasioned a waste of a like amount, against which there would not have been the smallest strategical set-off, nor any other compensation of any kind, except a slightly increased commercial convenience, not exceeding in capitalised value £17,000 at the outside.

Soudan Railway.—Perhaps one of the most important lines of railway communication now in course of construction is the Soudan Railway, running up the Valley of the Nile, in Egypt, and destined not only to open up the rich country traversed by that river, but eventually it will also doubtless form a very important rival to the Suez Canal route to India, as it will, when completed, shorten the length of the journey by three days. In consequence of the hard rocky nature of the ground, in many parts, the proposal to canalise the Nile so as to form a continuous water-communication past the great cataracts, as was proposed by Mr. Hawkshaw in 1865, will not be adopted. According to the plan proposed by Mr. Fowler, and now under construction, the first cataract will be passed by a ship-incline of 2 miles in length, to be worked by hydraulic power; and at the second, or great cataract, a line of railway—560 miles in length—will open up communication to the Soudan country, and this will eventually be extended to Massowah, on the Red Sea, a further distance of 430 miles. This new route will be altogether 1900 miles in length. Commencing at Alexandria, on the Mediterranean, the existing railways terminating at Roda will cover 310 miles of the distance. At Roda the passengers will be transferred to light and swift steamboats, and for 600 miles southwards the Nile will form the highway for inland traffic. In this distance the first cataract has to be passed, at which there is a difference in level of about 12·5 feet at high, and 15 feet at low Nile. This, as we have said, is to be passed by the construction of a ship-incline, nearly 2 miles in length, on the right bank of the river, commencing at the bottom of the cataract between the island of Sehayl and the river-bank, and terminating on the higher level in the harbour of Shælall, north of the celebrated island of Philæ. Rails will be laid on the incline, and suitable carriages constructed to run upon them. The vessel to be raised or lowered will be floated upon these carriages or cradles, the ship and carriage being then drawn over the incline by hydraulic engines driven by water, at high pressure, pumped into huge accumulators, at the summit of the incline, by a pair of large water-wheels placed upon pontoons and moored in one of the rapids of the cataract. A speed of from 4 to 7 miles an hour will thus be imparted to the vessel, according

to the height of the Nile and weight of the vessel. Thence the river communication will extend to Wady Halfa, the commencement of the Soudan Railway. A transference from steamboats to railway will take place at this point, and the 560 miles of the Soudan Railway will extend to Skendy. From Skendy to Massowah, the port on the Red Sea where the sea-passage will be again resumed, is 430 miles, which will be accomplished by an extension of the Soudan Railway. The gauge fixed on for the railway is 3 feet 6 inches.

Rail Economy.—In December last a paper, by C. P. Sandberg, was read before the American Society of Civil Engineers, in New York, upon "Rail Economy," in which—under the three heads Iron Rails, Steel Rails, and Traffic Capacity—the author dealt with the saving that might be effected in the item of railway cost. It was remarked that the late increased expense of iron added to the cost of railway construction, and tended to reduce the quality of rails; that Welsh rails were now often inferior in quality, but in the Cleveland district rail-making had greatly improved, chiefly by the increased application of fettling in the puddling-furnace. No late improvement, it was observed, promised so much to perfect iron rail-making as mechanical puddling, which now seemed to be an entire success. The demand for steel rails has become so great that they can now hardly be obtained at any price, whilst the supply is also limited by the lack of ore free from sulphur and phosphorus. The Siemens-Martin process of steel-making is declared to be superior to the Bessemer process, as it requires a less pure ore, but it has thus far proceeded so little that it can hardly be called a source of supply in the great market. Usually a steel rail will carry one-fifth more dead load than an iron one; hence, for the same traffic, the steel rail, in comparison with the iron, should not be reduced in weight more than 20 per cent. The weight passed over good iron rails, before rejection, has been found to average 10,000,000 tons, which may be taken to represent the life of extra iron rails, and six times that the life of good 56-lb. steel rails. On the London and North-Western line steel rails have lasted twenty times as long as iron; and on the Metropolitan Railway, with the greatest traffic in the world, where iron would not have lasted six months, steel will stand from three to four years. Prof. Rankine says the weight of the rails per yard in length should equal fifteen times the greatest load on the locomotive-drivers in tons. Perdonnet, in France, takes twelve in place of fifteen. The author of the paper, by adopting a section which permits a fish-joint stronger than the others in general use to be made, takes ten and less; thus for a 60-lb. rail the weight on drivers is put at 6¼ tons. Fish-plates of steel will enable rails to carry from 15 to 20 per cent greater load than if iron were used of the same section.

GEOLOGY.

Obituary.—The Rev. Adam Sedgwick.—Geological science has expanded so much during the past fifty years that it is difficult for any one man to be master of all the subjects it embraces. Sir Charles Lyell has expressed the difficulty he has felt from year to year in keeping up with its progress, and no man has done more to further the advancement of geology than he, by presenting the principles and general results of the science before the public. We have very few of those veteran geologists left who connect, as it were, the early history of geology with its present advanced state, who have contributed most largely to lay the foundations (which are lasting monuments to their honour) to which the geologists of the present day are adding detailed work—and there is plenty of that to be done.

The Rev. Adam Sedgwick, who died on the 27th of January last, at the advanced age of 87, was one of those veterans who helped to lay the foundations of geological science, and who is therefore intimately connected with its progress. Although for some years past he took no very active part in the furthering of geology, he yet remained until death at his post of Professor of Geology in the University of Cambridge, which post he had held since the year 1818, when he succeeded Professor Hailstone.

At this period little was known in England of geological science, but a

general notion prevailed, agreeing closely with the theory of old Dr. John Woodward, who founded the chair, that all fossils were the result of a universal deluge which had once swept over the whole earth, and to the agency of which all the strata owed their origin.

Professor Sedgwick directed some of his earliest inquiries to the structure of Devon and Cornwall, in which counties the relations of the rocks present problems of great difficulty—even now much discussion takes place in regard to them, as was pointed out in the last number of the "Quarterly Journal of Science." Professor Sedgwick, sometimes accompanied by Sir Roderick Murchison, examined the district in great detail, and determined, if not the true age of the beds, their true succession.

Professor Sedgwick devoted his attention at times to the Continent, and explained the geological structure of the Alps and Rhenish provinces. In the "Geological Magazine" for April, 1870, there was a biographical notice and a portrait of this eminent geologist: in the former was a list of forty-four papers contributed by him, a few in conjunction with Sir R. Murchison or Mr. W. Peile—all contributions to geological science. Among these we may mention his Memoirs on the Magnesian Limestone of the North of England; on the Trap Rocks of Durham and Cumberland; on the Fossiliferous Strata of the North of Scotland, and on the Isle of Arran; on the Mountains of Cumberland and North Wales; and his Essays on Slaty Cleavage.

These show the extent of country examined by Professor Sedgwick, and the many subjects he was master of.

No member of his University has contributed in a higher degree than he to elevate its character as a school of the natural sciences, and many of our leading geologists owe their first geological lessons to Sedgwick, who as a lecturer was clear, earnest, and philosophical, full of energy, and even to the last vigorous, and, when his health permitted, cheerful and full of humourous anecdote.

To Professor Sedgwick also the University is indebted for much care and liberality in providing for the now large collections of the Geological Museum, the nucleus of which was Dr. Woodward's own small cabinet.

It is some satisfaction to learn that the post of professor of geology in the University of Cambridge left vacant by the decease of the venerable Sedgwick, has been filled by a distinguished pupil of his—Mr. T. McKenny Hughes, M.A., F.S.A., F.G.S., of the Geological Survey of England and Wales. Mr. Hughes has done much detailed field-work on the geological survey in Kent, Hertfordshire, and in the Lake District. He has written portions of the Survey Memoirs illustrating the geology of the Lake District, and has also contributed largely to Mr. Whitaker's Memoir on the Geology of the London Basin. The "Quarterly Journal of the Geological Society," and the "Geological Magazine," contain papers by Mr. Hughes, and he is not only known as a clear writer, but as a ready and clear lecturer.

Geological Awards.—The awards of the Geological Society of London may be looked upon as the highest honours conferred upon geologists in this country, and they are intimately connected with the progress of the science, being either the reward of a life's devotion to its advancement, or a stimulus to one in early life to continue researches which have materially assisted the progress of geology. At the Anniversary Meeting of the Society, held in February last, the president, the Duke of Argyll, presented the Wollaston Gold Medal to Sir Philip Egerton, Bart., F.R.S., &c., for the services he has rendered to geology, during a period extending over forty years, in the special attention he has bestowed on the structure and affinities of fossil fishes and reptiles. The balance of the proceeds of the Wollaston Donation Fund was awarded to Mr. J. W. Judd, F.G.S., in recognition of his valuable researches in the Neocomian and Jurassic rocks of England, and in the Oolitic rocks of the west coast of Scotland. The Murchison Medal, the first award made under and in fulfilment of the will of the late Sir Roderick Murchison, was handed to Mr. William Davis, of the British Museum, in recognition of the services he has rendered to palæontology, in the skill and knowledge he has displayed in the reconstruction of extinct forms of life; and the balance of the Murchison Fund was awarded to Prof. Oswald Heer, of Zurich, for his researches in fossil botany

and entomology, and particularly for the light he has thrown upon the Miocene Flora.

Stratigraphical Geology.—Attention has been directed, particularly on the Continent, to deposits which fill up gaps in the table of strata. England is no longer considered as forming an epitome of the geology of the world, and yet gaps are being filled up in it, rather than new unconformities made out. On the Continent, the Tithonic stage of Stramberg forms in places a gradual passage between the Neocomian and Jurassic strata, which elsewhere, as in Northern Germany, Yorkshire, and Lincolnshire, are uncomformable.

Some discussion has taken place in regard to the Punfield formation, named by Mr. Judd. This he regarded as Neocomian, though still closely connected with the Wealden, and, in fact, forming a transitional series of beds between the two, though absolutely belonging to neither, and therefore worthy of a distinct name. Mr. Meyer, who recently read a paper on the subject before the Geological Society of London, repudiates the distinctive name, and includes the Punfield beds in the lower Greensand.

The Midford sands, so called by Prof. Phillips, occupy an intermediate position between the Inferior Oolite and the Upper Lias clay, but for a long time their true position was obscured by the appellation of either Upper Lias sands, or sands of the Inferior Oolite, according to the formation to which the writer inclined to consider them as more closely related. The term Midford sands, therefore, removes a good deal of misunderstanding, and its adoption will cause the neutral position of the beds to be better recognised.

In the same way the discussions as to whether the Rhætic or Penarth beds of England belonged more closely to the Lias or the Keuper are rendered needless, when both their stratigraphical and palæontological features are taken into account. It is now known that they present a gradual passage between the two, and although but a feeble representative of the beds developed on the Continent, they are yet a complete series in our country, and, as such, link the Keuper and Oolitic rocks in one conformable series.

It is becoming more evident that the sequence of beds which holds good in one place requires some modification in another. Like sedimentary conditions certainly did not always prevail over very large areas, while the organic remains will vary in a measure according to the different physical conditions which prevailed. Thus our Oolitic system varies greatly in its extension from Yorkshire and Lincolnshire, through Northamptonshire and Oxfordshire, to Gloucestershire, Somersetshire, and Dorsetshire. The divisions of the Northamptonshire Oolites have received a great deal of attention recently from Mr. S. Sharp and Mr. J. W. Judd, and the former geologist has this year given a second paper to the Geological Society of London on the subject. He showed that the series of beds were as follows:—

Great Oolite	Clay	
	Limestone	
	Upper Esturian Clays.	
Inferior Oolite	Lincolnshire Limestone (present only as a thin band in the north-east portion of the district.)	
	Lower Estuarine beds	Northampton Sand.
	Feruginous Beds	
Upper Lias	Clay.	

He considered that the great Oolite clay represented the Forest Marble and the Bradford clay of the West of England; that the Great Oolite limestone was nearly equivalent to the Great Oolite of Bath and the Cotteswolds; that the Upper Estuarine clays were identical with the Stonefield slate of Oxfordshire; that the Lincolnshire limestone was nearly synchronous with the grey limestone of Yorkshire (Inferior Oolite), and probably with the lower portion of the *Am. Humphriesianus* zone of the West of England, but extending a little below this zone; that the Lower Estuarine answered to the Lower Plant Shale of Yorkshire, but had no representative in the west; that the upper portion of the ferruginous beds of the Northampton sand was nearly upon the

same horizon as the Glaizedale and Dogger beds of Yorkshire and the *Am. Murchisonæ* zone of the west; and that the lower portion of the Northampton sand was represented by the *Am. opalinus* zone and the Midford sand.

Palæontology.—Mr. S. H. Scudder has recently described a new fossil butterfly (*Satyrites Reynessii*), from a tertiary deposit at Aix, in Provence. The fossil is a natural imprint, and its state of preservation shows that it had undergone great maceration in quiet water before being covered up by the deposits which have preserved its most essential features. The insect is placed on its side, with the wings elevated one against the other, the legs spread out as if it were suspended, the spiral proboscis unrolled, and the antennæ lowered in the same direction as the legs. The nearest living representative of this fossil butterfly are natives of India.

Mr. A. G. Butler has described, in the "Geological Magazine" for January, a new fossil butterfly (*Palæontina Oolitica*) belonging to the family *Nymphalidæ*, This insect belongs to a group completely tropical, its nearest allies being tropical American genera. It is interesting as belonging to the highest family of butterflies, and to a sub-family intermediate in character between two others—namely, the *Satyrinæ* and *Nymphalinæ*, but it is especially interesting as being the oldest fossil butterfly yet discovered.

Prof. O. C. Marsh, who has added largely to our knowledge of the fossil vertebrata of North America, announces the discovery of fossil quadrumana in the Eocene strata of Wyoming. These remains closely resemble, especially in many of the larger bones, some of the Lemurs, while the anterior part of the lower jaws is similar to that of the marmozets. The teeth are more numerous than in any known quadrumana.

Prof. Marsh also describes a new carnivore from the tertiary strata of Wyoming, named *Oreocyon latidens*. The remains indicate an animal about as large as a lion: the canine and premolar teeth of the lower jaw somewhat resemble those in the hyæna, but there were only two incisors in each ramus.

One of the most interesting discoveries of recent years has also just been made known by Prof. Marsh. It belongs to a new sub-class of fossil birds (*Odontornithes*). The remains were discovered in the Upper Cretaceous Shale of Kansas. The type species of this group is called *Ichthyornis dispar*: it has well-developed teeth in both jaws, very numerous, too, and implanted in distinct sockets. The possession of teeth, and also of bi-concave vertebræ, imply that these remains cannot be placed in the present group of birds, and hence a new sub-class has been formed. The bird was no larger than a pigeon; it was carnivorous, and probably aquatic. The fortunate discovery of these interesting remains is an important gain to palæontology, and does much to break down the old distinctions between birds and reptiles, which the *Archæopteryx* has so materially diminished.

Prof. H. A. Nicholson has described a new genus of Tubicolar Annelides, (*Conchicolites*) for some forms found growing upon the shells of *Orthocerata* in the Lower Silurian rocks of the north of England. The tubes in this genus agree with those of the modern *Serpulæ* in being calcareous, and they differ altogether from those of the extinct genus *Cornulites* in being altogether destitute of any cellular structure.

The affinities of *Calceola sandalina*, one of the most problematic of fossils, have been lately discussed by the Rev. T. R. R. Stebbing. It has been referred to the Conchifera, Rudistes, and Brachiopoda, but Mr. Stebbing agrees with Suess and Lindström in placing the genus among the Zoantharia rugosa. He refrains from giving the convenient name of coral to the *Calceola*, because Lindström, following Agassiz, gives reasons of some weight for separating the Rugosa from the true polypes, and conjectures them to be allied rather to the Hydrozoa than to the Anthozoa.

Dr. Anton Fric has described a new crustacean from the Polirschiefer, near Bilin, in Bohemia, which he names *Palæmon exul*. The discovery is remarkable, on account of its being a marine crustacean in a fresh-water deposit.

Mr. H. Hicks has described a number of new species of fossils from the Tremadoc rocks of St. David's, South Wales, which prove that these rocks are nearly allied to the lower part of the Tremadoc rocks of North Wales. The

discovery of a number of well-marked species of *Lamelli branchiata* in beds of an earlier date than those in which their presence had previously been known is of great interest.

Mr. Henry Woodward has described a new genus of shore-crabs, *Litoricola*, from the Lower Eocene deposits at Portsmouth; and a new trilobite, *Encrinurus cristagalli*, from the Cape of Good Hope.

From a letter we have received from Prof. Piazzi Smyth, the Astronomer-Royal for Scotland, we find that there has been a good deal of exploring work going on lately at the Great Pyramid. An explorer has been found in Mr. Waynman Dixon, a young engineer of Newcastle-on-Tyne, who has been in Egypt for nearly a year and a half, building an iron bridge across the Nile at Boolak, opposite to the Pyramids. He went out well read up in the new scientific theory of the Great Pyramid, and most earnest to promote its development, and has utilised all his spare time towards that end—latterly indeed, after his bridge was finished, going out to the Pyramid hill, living in the tombs there to be close to the spot, and attended by our old Arab servants as well as some of his own English workmen. His elder brother, Mr. John Dixon, of Cannon Street, London, occasionally went out to help, and some companionship was afforded during part of the time by Dr. Grant, an English physician in Cairo, but the soul of the whole movement was Mr. Waynman Dixon himself. In company with his head carpenter, "Jim Grundy," Mr. W. Dixon has taken casts of critical portions of the coffer, also of the "boss" on the granite leaf, has observed thermometers extensively, and taken several important re-measurings. He has also been boring away in divers places, *hoping* to find another chamber; but neither chamber nor passage has he yet met with, though in the Queen's chamber he has discovered the inner ends of two small channels like those in the King's chamber. They are rectangular, $9'' \times 8''$ nearly, go back horizontally about 7 feet, and then rise at an angle of about 32°, and go no one yet knows where. These channels had not been recognised before, as this outcrop into the Queen's chamber had been neatly filled up with a thin plate of white stone, looking superficially like the rock of the walls. One of them is in the north wall, and the other in the south. Inside them were found squeezed out flakes of white mortar (since then analysed by Dr. Wallace, of Glasgow, and found to be not carbonate, but sulphate, of lime), an ordinary "miva" stone-ball weight of the ordinary old "profane" Egyptians, a little bronze sort of grapnel hook, and a little staff of trimmed cedar-like wood a few inches long, but nearly perished. These channels it is proposed to call Dixon's channels. Outside the Pyramid, Mr. W. Dixon has discovered the finest specimen of a loose casing stone of the Great Pyramid known to exist; and he has also, in company with Dr. Grant, made a grand expedition into the Libyan Desert to examine the supposed Pyramid there, hitherto called Dr. Luder's Pyramid. It turned out to be no artificial structure at all, but a natural hill of a conical shape, and near it were abundant remains of silicified tree-trunks lying here and there, with petrified shells and jasper pebbles. From what has been done we may gather—

(*a*). The casing stone fragment has five worked surfaces, and two of them being ends, we can measure for the first time the *length* of a casing stone of the Great Pyramid as well as its angle; and what is its length? Twenty-five inches and a fraction, or the sacred and scientific cubit,—not of the profane and idolatrous Egyptians, but of Noah, the Hebrews, and the anciently *concealed* parts of the Great Pyramid.

(*b*). Two measures were made in the extreme passage by Mr. W. Dixon, Dr. Grant touching a line on either wall, supposed to have been drawn for an important purpose by the very architect of the primeval monument himself, and requiring, according to the scientific theory, to show a distance of 2170 inches from a crucial part of the interior. The result along the east wall shows 2170·5, and along the west wall 2170·4, when used in conjunction with Prof. Smyth's measures in "Life and Work," printed long before that theoretical conclusion had been thought of.

(*c*). Mr. John Dixon sent an account and drawings of the findings in Dixon's channels to the "Graphic" newspaper. So far so well. But he also sent

drawings of well-known parts of the Pyramid,—hand drawings,—and very accurate ones too, of parts of the Pyramid already *photographed*; and thereby he destroyed knowledge, and retarded the development of the public mind.

(*d*). Prof. Smyth weighed the grey granite ball and found it 8320 grs.; and therefore declared it to be a profane Egyptian miva belonging to some of the hod-men about the Pyramid; Sir G. Wilkinson having already published the weight of the Egyptian miva at 8303 grs. But he advised Mr. W. Dixon to have it authoritatively weighed by the "Warden of the Standards."

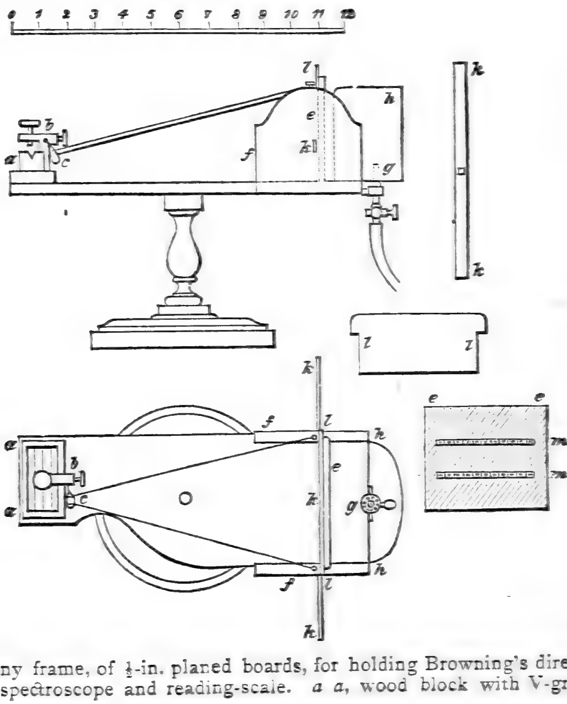
(*e*). The Warden of the Standards did so, made the weight 0·03 gr. different from Prof. Smyth's, and then wrote a letter to "Nature," December 26, describing the whole affair on one side, giving a wandering anachronical conclusion of his own, that the ball *may* have been an Egyptian *miva*, without saying a word of Prof. Smyth's conclusion; and then, worse still, declaring that Sir. H. Jarvis's pamphlet in 1869 was the *latest* and most satisfactory account of the length of the Great Pyramid's box-side, and advocating both his (Sir H. J.'s) mistaken reading of Herodotus and his garbled version of the result of direct measures.

(*f*). Two letters have been sent by independent parties to "Nature," pointing out the errors of the Warden of the Standards, but its editor has refused them both insertion: and, consequently, a third party has sent off a letter to "Les Mondes" in Paris.

LIGHT.

Mr. H. R. Procter has described to the Newcastle Chemical Society a glass reading-scale for direct vision spectroscopes. The apparatus consists of a

Fig. 1.



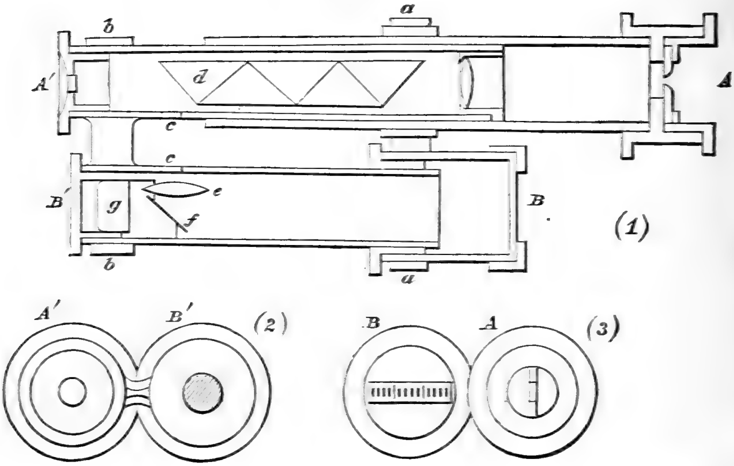
mahogany frame, of ½-in. planed boards, for holding Browning's direct vision pocket spectroscope and reading-scale. *a a*, wood block with V-groove for

holding the spectroscope; *b b*, brass clamp and screws for fixing it in its position, and for holding the wire ring, *c c*, to stretch the wire supports, *d d*, of a black curtain for keeping out stray light from the side-hole of the spectroscope; *e e*, photographed glass millimetre, or other scale of equal parts, sliding between two upright mahogany cheeks, *f f*, and lighted from behind by the gas-jet, *g g*; *h h*, a tin screen to shade the flame; *e e*, the dark-glass scale with transparent graduations at *m m*, seen in the spectroscope above and below the spectrum; the upper one can be shut off by a black card screen, *l l*, slipped down in front of the glass plate; and all but three scale-divisions of the lower one, at any point, can be stopped out by a card slider, *k k*, slipping through slits in the two side-cheeks of the frame, to confine the vision to the graduations closest to any line. The glass plate is backed behind by a piece of oiled paper gummed by its edges to the photographic plate. Prof. Herschel stated that he has also made an addition to Browning's pocket microscope. He put the thinnest possible film of mica on the glass plate which fitted the eye, and he held it in place with a small india-rubber ring. That film of mica, by inequalities of its thickness, or other refracting properties which it possesses, eclipses certain rays of particular refrangibility, and, according to the thickness, it will eclipse more or fewer bands in the whole range of the spectroscope; so that, looking through it, you get the spectroscope divided into compartments by dark bands. As these bands are not easily seen, he thought he would bore a hole in the side of the spectroscope, and get a scale reflected in its prisms; and if this scale was to be a pocket one, he must, in this hole, which was bored through, put a small lens, and fix the scale close in front of that lens, for use as a scale of reference. The use of a spectroscope resolves itself into placing every line according to its scale position on any arbitrary scale, but as far as possible on the scale of what is called the natural standard scale of wave-lengths. If the position of every line which is mapped can be given in wave-length, its description is then intelligible to everybody. The scale fitted to this spectroscope, of uniform intervals, would enable spectra to be recognised; but being a new instrument its indications first require to be reduced to some well-known standard. The simplest way of recording them would be by wave-lengths. The positions of successive lines, as seen in the common spectroscope, are not in the simple proportions of their wave-lengths; the blue lines are more spread out than corresponds to the differences of their wave-lengths, and the red lines are nearer together, while the wave-lengths in the latter part of the spectrum are far apart; and therefore the question was how to pass from a uniform scale used in a spectroscope, in the manner shown by Mr. Procter, to the scale of wave-lengths. He had quite recently found that a uniform scale, used with any spectroscope, like that divided in Mr. Procter's instrument into millimetres, is approximately proportional to a scale of inverse fourth-powers of the wave-length. If, for example, we take the readings of the sodium line, and of any other known line of the spectrum, on a scale of equal parts, and replace them and all the other readings of the scale in proportion by the inverse fourth-powers of the wave-length, we would then find that we can pass from the uniform scale to the wave-length by taking the inverse (or reciprocal) of the readings so replaced, and taking the fourth-root of that reciprocal to obtain the wave-length. It so nearly was the case in all prisms of ordinary dispersion, as to present an almost accurate means of passing without trouble from the uniform scale of the spectroscope to the wave-lengths; and although it is not quite true of the whole range of the spectroscope, yet if used between the short interval of two neighbouring lines to find the wave-length of an intermediate line, it will give the wave-length of that line directly from the milimetre scale.

The following is a description of Procter's direct-vision micrometer scale for pocket spectroscopes. A A′, small direct-vision spectroscope. B B′, a parallel brass tube (first slide of a miniature toy telescope) braced to the tube and draw-tube of the spectroscope by the double rings of bent copper plate, *a a*, *b b* (seen also in Fig. 2). The ring, *a*, slides on the main tube of the

spectroscope by a collar of soft leather interposed between them. *cc*, small holes about one-eighth inch diameter drilled opposite to each other in the brass tubes for viewing a magnified image of the reading scale, B, reflected in the prism face, *d*. *e f*, a lens of short focus, and thin silvered glass diagonal

FIG. 2.



mirror, cemented on a cork, *g*, in the stopped eye-end of the telescope tube, for obtaining a magnified view of the glass reading scale at B. The latter is marked in transparent lines on an opaque ground, as shown in Fig. 3, and is illuminated by the same direct light as that of which the spectrum is observed.

We are indebted to Mr. R. C. Johnson for an account of a curious physical phenomenon witnessed at Ziza—a lunar dew-bow. Ziza is a ruined city which is situated about 20 miles E. of the northern part of the Dead Sea on the table-land of Moab, and is about 3000 feet above the sea-level. The Moabite Expedition was camping there on the night of the 24th February, 1872, close to a large reservoir. Mr. Johnson says, "It was full moon (at 11 a.m. of the same day), and having seen ducks come down to the reservoir, we turned out about 8 p.m. to lie in wait for them against the sloping banks of the reservoir. An *exceedingly* heavy dew had fallen, and I noticed when walking with my back towards the moon that I was preceded by a faint circular halo (extent of circle about $\frac{1}{8}$ of circumference); the origin of which at first was rather puzzling. On attentively considering the position of my eye and the halo with regard to the moon, I found that it was exactly at the angle required for an inverted rain-bow, and that it must really be a dew-bow. It seemed brighter than a lunar rain-bow, which I have once beheld. Dr. Tristram also noticed it after his attention had been called to it. I may also state that a similar thing was seen in sunlight when a very fine dew was thickly spread upon some large webs made by caterpillars in the same country. This was looked out for after having previously seen the lunar dew-bow.

An instrument invented in Germany for testing colour-blindness consists of a rotating apparatus, which moves a disc whose centre is a circle, one half black and the other white. Outside of this is a ring half red and half green, then another ring of violet and red, then the outside ring of violet and green. When rapidly rotated, the centre appears to be coloured grey, that is black and white mixed. To a green-blind person, the middle ring will appear grey,

that being a result to him of a mixture of violet and red. The outer ring will appear grey to a red-blind person, and the inner one grey to a violet-blind.

Microscopy.—A new form of pocket microscope has been contrived by Prof. G. T. Brown of the Royal Veterinary College. In general construction it somewhat resembles the well-known clinical microscope of Dr. Beale, but is very much smaller, the extreme length being only 3 inches, and packed in its case with two slides and some thin glass measures $3\frac{1}{4}$ inches in length by 1 inch wide, and $1\frac{1}{8}$ inches in depth. A case double in width will hold, in addition, an extra eye-piece, two objectives in boxes, a glass tube, and two dissecting needles. The short body necessarily involves a loss of magnifying power; this is, however, met by using a very deep eye-piece, the instrument with an E eye-piece giving the same power as an A eye-piece with a 10-inch body. The fine adjustment is made, as in Dr. Beale's instrument, by sliding the draw-tube containing the eye-piece, a mode of adjustment characterised by its extreme sensitiveness. It is capable of working with powers as high as an immersion twelfth. The instrument has been especially designed for observations in veterinary practice, where it is necessary to have the microscope ready for use at any place: for this purpose it is especially suited, as packed in its case, it is less in dimensions than any case of surgical instruments. It will no doubt prove very useful to field naturalists, and often prevent worthless gatherings being brought home by permitting examinations to be made on the spot. The only disadvantage of the microscope is, that owing to its small size, the objectives and slips of glass are of corresponding dimensions. This prevents the utilisation of the observer's stock of objectives, and also the examination of objects mounted on the usual 3×1 slides.

At the suggestion of Dr. Pigott, a micrometer scale has been ruled by Mr. Ackland on the flat side of a plano-convex lens of very long focus; this is inserted into the diaphragm of the eye-piece. The definition of the object is less impaired than when the old form, having several plane glass surfaces, is used. It is also easier to make with accuracy, as there is always more or less difficulty in working perfectly true parallel surfaces. It will doubtless prove of equal value for dividing the field of the microscope into a number of squares for the purpose of making drawings, a favourite method with some observers, and to whom better definition than with the original plane disc will be a welcome improvement.

Some guide to beginners in the use of the micro-spectroscope has long been a desideratum; this want is likely to be supplied. An introductory work on the subject, with numerous figures of absorption-spectra, lithographed by the author, is reported to be in progress. Work in this department has been much hindered by the want of such help, many instruments being almost useless to their owners for lack of some practical hints as to their employment. The subject is one which has a future before it; little or nothing is at present understood as to cause of the remarkable phenomena of the absorption-spectrum, and there is plenty of work, with the probability of valuable discoveries, for those who apply themselves to such researches.

Mr. T. Johnston English has brought before the Quekett Microscopical Club a new apparatus for injecting animal tissues for microscopical purposes. The instrument consists of a Woulfe's bottle with three necks. No. 1 is fitted accurately with a cork, through which passes a glass tube about the diameter of a goose-quill, one end of which reaches to the bottom of the bottle, and to the other bent end is tied about 12 inches of india-rubber tubing of the same diameter. The glass tube is made perfectly air-tight in the neck of the bottle by sealing-wax varnish, and the india-rubber one is closed by a pinch-cock. In No. 2 neck is placed a simple contrivance which answers the purpose of a condensing syringe. It consists of a piece of glass tubing 5 or 6 inches in length fixed air-tight in the cork; to its upper extremity is attached a small india-rubber ball, having a small hole in one side, and to the lower end a small oil-silk valve, like those used in air-pumps, opening downwards. The third neck is closed by a cork, and serves to introduce the injecting fluids. To use the instrument, the proper sized nozzle is fixed on the india-rubber

tube, which is closed by the pinch-cock. The requisite quantity of fluid is then poured into the bottle, and the cork firmly inserted. Pressure is now made on the india-rubber ball, taking care to close the hole with the finger. By this means air is forced down the tube through the valve into the bottle. On removing the pressure from the ball, the valve closes and the ball is re-filled through the hole in its side, and the compression can be increased to the necessary extent. The pinch-cock is now cautiously opened, and the fluid rushes up the tube completely filling it and the nozzle; the cock is then closed, preventing further exit, and the instrument is ready for use. The nozzle is introduced and tied into an artery in the same way as with the ordinary syringe. The inventor prefers glass nozzles to those of metal, as they are lighter, can be made very easily, and drawn out to very fine points. The instrument has the advantage of being self-acting, and leaving the operator the free use of both his hands, besides being more regular in its action than the usual syringe, saving in the hands of those well practised in its use. A form of apparatus nearly similar is employed by Dr. Rutherford; in this, however, the pressure is obtained by means of a column of water.

The subject of mounting objects in the dry way has received some attention from Mr. W. Ackland, F.R.M.S. Where thin cells are required, he employs a ring of the varnish already mentioned in this Journal (vol. ii., N.S., p. 271); this is allowed to become throughly dry before being used, a number of cells being made and kept in stock. Where thicker objects are to be mounted, metal or glass cells are used, coated on their upper surface with the same varnish. The cover is fixed by being clipped to the cell; heat is then applied, and the varnish softened. Upon cooling, the cover is securely fastened. Where from the nature of the object the slide cannot be safely heated, a thick disc of brass sufficiently heated is applied to the cover with the same result. For attaching delicate objects to slides, the following preparations are used:—

No. 1. Quinine Solution—

Sulphate quinine	5 grains.
Acetic acid	5 drops.
Water	1 ounce.

No. 2. Gelatine Solution—

Gelatine	20 grains.
Glycerine	5 drops.
Quinine solution as above ..	1 ounce.

No. 3. Gum Solution—

Gum tragacanth	5 grains.
Glycerine	3 drops.
Quinine solution	1 ounce.

A small portion of No. 2 or No. 3 solution is spread upon the glass before the cell is made. When the objects are to be attached to the glass they are placed on the prepared gum or gelatine surface, and the slide placed under a bell glass containing a saucer of water; this softens the coating, and attaches the minute objects. They are then dried under another bell glass, the moisture being absorbed by chloride of calcium. The covers are then fixed as before mentioned. The quinine solution may be used to prevent mouldiness in paste or gum, and is an advantageous substitute for the bichloride of mercury usually employed for this purpose, as the mixture is not poisonous.

Mr. F. H. Wenham has communicated to the Royal Society* the formulæ for the improved microscopic objectives recently constructed by him. His paper contains an account of the successive improvements in the construction of object-glasses. A little before the year 1829, the three superimposed achromatic lenses appear to have been in use; but no knowledge of their properties had as yet been arrived at, and it was not until Mr. J. J. Lister announced his discovery of the *aplanatic foci* to the Royal Society in the same year that any marked improvement took place. In the year 1831, the late Andrew Ross successfully constructed an object-glass on this principle, and at

* Proceedings Royal Society, October 31, 1872.

the same time made the discovery of the aberrations caused by covering the object with a film of glass, and applied the means for their correction. Mr. Wenham then described the increase of aperture obtained by the use of the *triple front* lens. This was followed by Mr. Lister's introduction of a *triple back* lens in the year 1850, by which the aperture of the $\frac{1}{8}$th was increased to 130° or more, and this form was employed for high powers until quite recently. The next advance was Mr. Wenham's discovery of the feeble correcting power for colour of the *flint concave* in the triple front, and his successful substitution for it of a thick single lens, the form now usually employed in the best high-power objectives. His attempt was to substitute a single lens in place of the middle doublet. This, however, for reasons shown in his diagram, failed to produce the desired result, and led to the employment of a triplet between two single lenses. By this means perfect corrections were obtained, and the construction of the object-glass much simplified, there being only ten surfaces and but one concave of dense flint used in correcting four convex lenses of crown glass. Mr. Wenham in planning an object-glass prefers constructing a diagram on a large scale, as being far less intricate than mathematical calculation. The paper is very fully illustrated, and will be duly appreciated by all interested in the construction of objectives, as it forms an admirable sequel to the valuable series of papers by the same author in the first volume of the "Monthly Microscopical Journal."

HEAT.

Professor Wheildon, of Concord, U.S.A., advances, in opposition to what is known as the Gulf Stream Theory, an atmospheric theory to account for amelioration of climate and an open sea in the polar regions. The accounts of Arctic voyages show sudden rises of temperature when nothing but an unlimited extent of ice is near. These changes could not have been consequences of proximity of open water, which at the highest, would only be 29° of temperature. The theory of Professor Wheildon is that open, melting ice, rain after snow, and other phenomena in Arctic regions, are not caused by winds warmed by an open sea, but by a circulation of air in which warm winds descend from upper atmospheres; being a circulation by which winds heated at the equator reach the poles.

Having occasion to cool a red-hot copper ball, Mr. W. F. Barrett plunged it into a vessel of soapy water. The ball entered the water without any hissing or perceptible evolution of steam; and upon being removed seemed as brightly incandescent as before; other metal balls were then tried with the same result. The soapy water was then replaced by fresh; but upon plunging an incandescent ball into this, the hissing was loud and the evolution of steam copious. Mr. Barrett infers that the presence of soap in the water contributed to the formation of the spheroidal state. Further observation showed also that albumen, glycerine, and organic matters generally facilitated its occurrence. The author seeks to establish a possible relationship between this phenomenon and certain boiler explosions, from the possible entrance into boilers of oil or other organic matter.

The Earl of Rosse has communicated to the Royal Society a paper "On the Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its variation in amount with her Phases," in which he gives an account of a series of observations made in the Observatory of Birr Castle, in further prosecution of a shorter and less carefully conducted investigation, as regards many details, which forms the subject of two former communications ("Proceedings of the Royal Society," vol. xvii., p. 436; xix., p. 9) to the Royal Society. The observations were first corrected for change of the moon's distance from the place of observation, and change of phase during the continuance of each night's work, and thus a curve, whose ordinates represented the scale-readings (corrected), and whose abscissæ represented the corresponding altitudes, was obtained for each night's work. By combining all these a single curve, and table for reducing all the observations to the same zenith-distance was obtained, which proved to be nearly, but not quite, the same as that found

by Professor Seidel for the light of the stars. By employing the table thus deduced, and also reducing the heat-determinations obtained on the various nights for change of distance of the sun, a more accurate phase-curve was deduced, indicating a more rapid increase of the radiant heat on approaching full moon than was given by the formula previously employed, but still not so much as Prof. Zollner's gives for the moon's light. By employing Laplace's formula for the extinction of light in our atmosphere the heat-effect in terms of the scale-readings was deduced, and an approximation to the height of the atmosphere attempted. From a series of simultaneous measurements of the moon's heat and light, at intervals during the partial eclipse of November 14, 1872, when clouds did not interfere, it was found that the heat and light diminish nearly, if not quite, proportionally; the minimum for both occurring at or very near the middle of the eclipse, when they were reduced to about half their amounts before and after contact with the penumbra.

ELECTRICITY.

M. Gramme's constant current magneto-electric machine is now applied to the purposes of lighting and electroplating. As our readers are aware, the arrangement of this machine permits of the obtaining of an electric current in one direction, perfectly continuous, and of great strength. Mr. Sabine, who has recently tested one of the smaller varieties of these instruments, states that it gave an electromotive force of 68 to 258 Minotti's cells with 60 to 230 turns of the handle per minute. The instrument in question was composed of 36 bobbins, each containing 180 yards of No. 40 copper wire, revolving before six magnets. One of these machines is shortly to be employed in the lighting of the clock-tower of the Houses of Parliament.

Nearly related to this instrument is that of M. Le Roux, for exhibiting a modification of Faraday's celebrated experiment with the copper-disc induction-apparatus. A disc of red copper, 15 c.m. in diameter and 2 m.m. in thickness, receives from a multiplying motion a rotary speed of 180 turns per minute as a maximum. This disc is arranged between two circular masses of soft iron, these masses being connected by a frame of soft iron, portions of the frame forming the core of four electro-magnets. The faces of the masses of soft iron thus acquire an opposite polarity. With this apparatus a bright spark can be obtained.

Mr. Willoughby Smith records a most interesting experiment relating to the conductivity of selenium, a metal of very high resistance. Mr. Smith took several bars of the metal, of from 5 to 10 c.m. in length and 1 to 1½ m.m. in diameter. Each bar was hermetically sealed in a glass tube, and had a platinum wire at each end for the purpose of connection. It was found that the resistance altered materially, according to the intensity of the light to which the metal was subjected. When the bars were fixed in a box with a sealed cover, so as to exclude all light, their resistance was at its highest, and remained very constant; but immediately the cover of the box was removed the conductivity increased from 15 to 100 per cent, according to the intensity of the light falling upon the box. Merely intercepting the light by passing the hand before an ordinary gas-burner, placed several feet from the bar, increased the resistance 15 to 20 per cent. If the light be intercepted with glass of various colours, the resistance varies according to the amount of light passing through. To insure that temperature in no way affected the experiment, one of the bars was placed in a trough of water, so that there was about an inch of water for the light to pass through. The results were the same. And when a strong light, from the ignition of a narrow band of magnesium, was held about 9 inches above the sealed tube, the resistance immediately fell more than two-thirds, returning to its normal condition immediately the light was extinguished.

Considerable excitement has prevailed in electrical circles, caused by a paper published by M. du Moncel, in "Comptes Rendus," as to the conditions of the maximum resistance. His conclusions—instead of indicating that, for a galvanometer to attain the best possible conditions of sensibility with regard to

a circuit of given resistance, it should be necessary that the resistance of the magnetising helix should equal that of the exterior circuit—have shown that the conditions of sensibility admit of a much greater length of wire than would correspond to the resistance of the exterior circuit. His calculations and experiments prove the maximum to be obtained with helices presenting twice the resistance of the exterior circuit.

M. Benoit has found the initial resistance in steel and iron to be doubled at 170°; in silver, copper, and gold, at 255°; in platinum, at 455°. In alloys the increase is generally more feeble; in standard alloy, for example, the resistance is increased at 860° by only 0·3 of the value at zero. The coefficient of expansion was carefully taken into account.

Dr. Blake, of the San Francisco Academy of Sciences, announces the discovery of a current of electricity running north and south, at a distance of about 150 miles from the Pacific coast, along a belt of metallic deposits, serving as a conducting-chain.

TECHNOLOGY.

It is estimated that there from 20,000 to 25,000 persons in Europe daily engaged in the preparation of hair and the manufacture of felt hats, in which processes they are exposed to mercurial poisoning. M. Hilairet, in experimenting on this subject, impregnated skins with a neutral substance, as molasses, or dextrine, or sugar, then put them in nitric acid, and found that by the action of the nitrous and hyponitric acids thus developed, the hair underwent a change of structure corresponding exactly to that obtained by means of the solution of mercury in nitric acid.

Von Scherzer, who first introduced to Europeans a varnish made by the Chinese, by beating together fresh blood and quick-lime, and used to make wooden articles completely water-tight, states that he has seen in Pekin wooden chests, which have been varnished with it, and after a journey over Siberia to St. Petersburg and back, were still sound and perfectly water-tight. Baskets of straw, used for the transportation of oil, are made fit for the purpose by means of this varnish; it also gives the appearance and firmness of wood to pasteboard coated with it. Articles required to be absolutely impervious are varnished twice, or at the most three times, by the Chinese.

M. Hallwachs asserts that not only green, but red carpets also contain arsenic, particularly the brilliant dark reds now so much in vogue. Samples of these carpets burned with the blue arsenic flame, gave off the characteristic garlic odour. Enough colour to give a distinct arsenic reaction could be rubbed off with the finger. A solution in hydrochloric acid produced the usual greyish precipitate of metallic arsenic.

A few years ago an oil well was started near Cumberland, Maryland; but instead of striking oil, the pioneers came upon a gas chamber and penetrated it. The gas was ignited and continued burning. About a year ago, Mr. Haworth, of Boston, purchased the well, and obtained a patent for the manufacture of carbon. The gas is allowed to burn against soapstone plates, on which the carbon is deposited in the form of soot. Six hundred and sixty burners are now in operation, each burner consuming 8 cubic feet per hour. By a mechanical arrangement, the soot is scraped and deposited in large tin boxes about 3 feet long, 1½ feet wide, and 1½ feet deep; scrapers are passed along the soapstone plates every twenty minutes, and the boxes are filled on their fourth passage. A building, twice the size of the present one, is now in course of construction. It will have in use 1328 gas burners. The present consumption of gas amounts to about one-twelfth the whole quantity escaping from the well. The total consumption of gas by the burners of both buildings will be one-fourth of the whole. The carbon is generally used for the manufacture of ink.

The following is a description of the process for preparing alcohol from sawdust:—Into an ordinary steam boiler, heated by means of steam, were

introduced 9 cwts. of wet sawdust, 10·7 cwts. of hydrochloric acid (sp. gr. 1·18), and 30 cwts. of water; after eleven hours' boiling 19·67 per cent of grape sugar was formed. The acid was then nearly saturated with chalk. Yeast was added after the saccharine liquid had cooled down to 30° C., and the fermentation finished in twenty-four hours. 26·5 litres of alcohol of 50 per cent at 15° were obtained quite free from any smell of turpentine, and of excellent taste. It appears that the preparation of alcohol from sawdust may be successfully carried on industrially when it is precisely ascertained what degree of dilution of acid is required, and how long the liquid has to be boiled to convert all the cellulose into sugar. Fifty kilos. of the sawdust yield 12 litres of alcohol at 50 per cent.

Messrs. Baerle, of Worms, have discovered soluble glass to be a valuable washing powder and detergent. Take 40 parts of water, at a temperature of 50° to 57° C., and 1 part of soluble glass; plunge the wool into the mixture, stirring it for a few minutes. Then rinse the wool in cold or tepid water; it will be found to be quite white and void of smell. Sheep also may be washed with the same preparation, care being taken to cover the eyes of the animal with a bandage, to perform the washing with the solution quickly, and to remove the surplus with tepid water. In the case of combed wool, it should be first steeped into the solution, and afterwards into another bath composed of 80 parts of water at 37° C., and 1 part of soluble glass. In this way the employment of soap or soda is not necessary. For laundry purposes a bath must be prepared over night with 20 to 30 parts of water at 50° to 57° C., and 1 part of neutral soluble glass; the linen is plunged into this bath and left until the following morning, when, after the bath has been re-heated with additional hot water, it is to be worked with a wooden stamp. The colour of the solution shows when the fabric is clean. The operation is completed by rising with a little soap; but it is well to pass the fabric again through a weak solution, consisting of 1 part of soluble glass to 50 parts of water at 45° to 50° C., and then to rinse in fresh water.

CHEMICAL SCIENCE.

A new burette has been lately used in Paris. It consists of an upright tube drawn out to a fine aperture below, like that of Mohr, and supported in the same manner. The opening at top is fitted with a perforated cork, through which plays a glass rod, reaching down to the bottom, and ground conically, so as to fit water-tight into the tapering delivery-end of the burette. A lateral aperture at the top serves to charge the instrument. This form is useful in working with solutions of permanganate of potash, or other reagents which attack the india-rubber which in Mohr's pattern connects the delivery-tube to the body of the burette.

MM. Samal and Berouson have recently patented a new method of bleaching animal textile fabrics, by means of a feeble solution of the sulphurets of sodium and potassium. These products remove the gum in preparing silk and in scouring wool. In the first case the bath should be boiling; in the second, the temperature of the alkaline sulphuret should not exceed 50° C. The more difficult it may be to remove the gum and prepare the silk, the less the solution should be sulphuretted; in some instances the protosulphuret may be employed. The aluminates of soda and potash have also been used in the same manner.

ERRATA.—P. 173, line 19 from bottom, for "*land*" read "*band*." P. 201, line 26 from top, for "*masses*" read "*maps*."

QUARTERLY LIST OF PUBLICATIONS RECEIVED FOR REVIEW.

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The Year-Book of Facts in Science and Art. By John Timbs. *Lockwood and Co.*

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PERIODICALS.

Naval Science.
The Popular Science Review.
The Geological Magazine.
The American Chemist.
The Westminster Review.
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The Civil Service Gazette.
Revue Bibliographique Universelle.

PROCEEDINGS OF LEARNED SOCIETIES, &c.

Monthly Notices of the Royal Astronomical Society.
Monthly Microscopical Journal. *Robert Hardwicke.*
Proceedings of the Royal Soeiety.
Ofversight af Kongl. Vetenskaps-Akademiens Förhandlingar. *Stockholm: Norstedt and Soner.*

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THE QUARTERLY
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JULY, 1873.

I. SECTS AND SCIENCE.

THE land is full of disputes about sectarianism, denominationalism, religion, and materialism, literature, and science. There was a time when we had only two choices—to believe as Romanists or Anglicans; gradually we rose to have three, and Churchmen and Nonconformists took each their place; but now the busy brain has split these into numerous parties, and the whisperings and breathings of history have passed through the stage of the Æolian harp, and risen into violent storms threatening to destroy. Galileo's reputed muttering is becoming one of the most powerful voices of modern times, and the terrible "still it moves" is, in some form or other, heard from the mouths of most scientific men, who threaten to make science a power in every department of government, and, as some of them suppose, in all things relating both to thought and action. We do not take the latter view, but we take (perhaps not all) the former. We do not believe that physical science will ever govern the whole world, or the lives of the best of men; we do not even believe that moral science will rule paramount, or any science whatever that we can understand, because we consider that there will be a movement forwards, always in advance of our reason. But no man who knows the force of natural truths can help distinctly wishing that man may rapidly be taught to see their beauty, and gain the power that lies within them to aid him in the labours of his life, as well as in all his thoughts and aspirations.

The hopes of humanity from natural science are high; and when we think of our ancestors wending through the rainy, roadless, and mud lands of Europe, with straw for their boots and their stockings, and of ourselves rushing in an express train, sleeping in an apartment heated with hot water, we have a foundation for our confidence. When we see the laws of health setting kingdoms in motion to stop by united action the plague nursery among the pilgrims to Mecca, and when we learn that civilisation may be promoted

amongst the inhabitants of the wildest districts, where communication by messengers is scarcely possible, a simple wire doing all the work, we begin to see that the great cities of the world are no longer to be the producers of invention, and the *foci* of movement, and that we may have these scattered over the world without the disadvantages of inordinate congregations of men.

We have the fullest faith in science, a faith which does not waver, but at present we shall not dilate upon it; we say it that we may more clearly object to that class of men who see also its beauty and its power, but have lost the knowledge of the fact that much beauty and power existed before it. We have a large class of men who know more or less of physical science, and having seen the exactness of many of its conclusions, look to its methods for deciding all questions arising among them. It is always amusing to see people with very narrow views—they are generally very exact within certain limits; very certain, and very determined; very active, and often very successful, because they see their end near, and have not far to go. But when we find that their certainty is akin to that of the boy who is sure that he will find the rainbow if he only gains the other hill, and when the means of attaining a great object are as small as the child's arms that stretch out for the moon, knowledge of failure is the only success to be hoped for, a knowledge that broadens.

Our novels are full of descriptions of the small sectarian who preaches his little belief in his little chapel, with little knowledge, to a small congregation; but we are not sure that such men are the narrowest. Our novels have not yet sought out the preachers of mere physical science, and explained the foundation of the truths so scantily dealt out by them. They have not yet learnt to laugh at a national faith consisting of geology, or astronomy, or mineralogy, or pictured the consolations of the soul fed upon chemistry and physics, or they would have shown how little these are able to fill the circle of all man's rational hopes, or even daily needs. The merely scientific man, whilst enlarging his own importance and diminishing that of others, forgets that he is simply doing that which he objects to in others, and is forming a sect, and as such, therefore, we paint him in our minds; and as we desire to be above mere sectarian views, we refuse to unite with him alone, but shall receive him as one of the many who preach to us daily their partial truths, and receive from us our partial assent.

It is our part to advocate the views of men of science so

far as to bring science into its proper position in deciding truth whenever it can decide, but in speaking to the young we must not be purely chemical or physical, we must remember to be men, and must contribute to education in such a way as to educate the young to become men also. We shall advocate no little doctrine of Little Bethel, or of Romanism, Anglicanism, or scientism, as the only sections in which truth are to be found, but we hope to be ready to receive it wherever it is. Devoted to physical science as we are, we should not suppose our sons to be educated by being continually in a scientific laboratory, any more than an intelligent minister of any religion would consider his children to be educated by having them confined solely to listen to the teaching from the pulpit.

The struggles of mankind to obtain knowledge have been long and various, and he only is educated as a man who has followed them with sufficient attention to enable him to learn the actual standpoint of humanity, and the method of arrival. We can imagine our ancestors coming out of the distant East, moving forward slowly towards Europe with their flocks and their wealth, staying centuries occasionally at a place because they liked it, and had few enemies, and then moving along to some more favoured spot when disturbed or becoming greedy of greater gain. Let us imagine one of them who knew of the whole road, and at last arrived at the rich lands of Normandy, or obtained the full throne of England, boasting of the steps he had made, and ridiculing the stupidity of his forefathers, perhaps Odin, who was satisfied with poor plains in the North of Europe, or some cold spot approaching to Scandinavia. It would be an empty boast that he was greater than Odin; the triumph may be gained by the least able if he only lives at the proper time for it. We hear our students of the present criticising the past with a lightness which is productive of smiles, and some of our scientific men are so elated with their position as the latest men upon the earth, that they would readily break off their connection with the past, and live as the men in whom wisdom had first grown. But they also will move to the past, and their wisdom will be part of the long line, and they will be mere individuals in the endless caravan which stretches from the beginning into the future. It is only when we consider the littleness of each that we can become truly wide or broad in our sympathies.

It might be worth while to enquire whether as a nation we are becoming so or not; probably we are broadening in

some respects and narrowing in others. If the individual is giving way to his littleness, is thinking too much of his own gains and his own happiness, if he is forgetting the past in the foundation of the present, and is weakening thereby the foundations of the future to be built on it, it is for the nation in its collective capacity, or for the wiser men, to lead the young. This must be so done that they shall not enter active life as the inferior animals do, with their mere instincts and unaccumulated knowledge. We see great danger of the latter; we see more than danger, we see men growing up in this condition of want of early experience to an extent greater than can be viewed without objection, although absolute loss is impossible in our busy world, where the most ignorant uses modern arts of civilisation. There is evidently a strong party in England determined to break off from the remotest contact with the traditions of the great eternity behind us, as they have ceased to think of that which is before us. They are men of observation mainly, and they have driven their principles to an extreme, and attempted to make their observations on that which is not present. The type of such men is easily seen in the less civilized state: in mercantile life they are men who drive little bargains, look after little gains, think a bird in the hand is worth two in the bush, and with a firm grip hold enough of the goods of the world to enable them to live without fear of starvation; wider minds come into the same field and become great merchants. It seems to us that we can detect the same or analogous smallness in the sayings and doings of those who learn only the practical arts of the scientific men of modern times, dealing only with physics. They have one mode of thought, clear, sharp, and beautiful, but they fail to look with the broad views of humanity, because they have not learnt how humanity thinks and feels; still they are often the cleverest and most inventive of men, and humanity will thank them for their discoveries, and by adopting them will give them the width of nature. This latter our great institutions, our universities, ought to look after; it is for them to think on every side of a question, and to reject nothing that humanity holds dear. Clearly, however, the small dealer has instilled into us many of his principles. We seek too exclusively to teach a boy that which will enable him to earn his bread; we do as the hens do, and set before them a few crumbs until they can find enough for themselves—we feed them as birds do their young, with worms, or with game, until they can fly after more; only a few can be taught to

fly about the heavens for joy, singing with the lark, or rolling like the tumbler. These are like arts that bring no bread, but typify leisure, grace, and overflow of life, thought, and feeling.

The wise men we ought to find in our universities. They must keep the links of humanity together; they must prevent us from looking at subjects from one point of space or time merely, and enable us to view them from every good loophole, even out of almost forgotten eyes of Pythagoras and Zoroaster, and others, up to our time. And when the world laughs at such names, as unfit to teach us to make a thousand pounds a year or a week, we shall say "These men saw a world that we do not see; and when our own view is rather confined, we may see it profitable to use their vision, and claim for cultivation the fields they discovered."

The universities must be broad, or why should we call them universities? Narrow them, and in that proportion you make them sectarian. They certainly began with very limited views, but they have gradually grown, and one or two include nearly all the circle of human thought. None, however, include actually the whole. When London University rose and excluded the religious element, that was a decided step in the formation of a sect. It limited the universality, so to speak, and although it may be said nominally to have excluded only one branch of mental activity, we must remember that the branch was to the most of the world the most important, and in early times the only branch taught at seminaries rising to universities. It was a new step separating, in a prominent manner, religious and secular education. That separation is going on still more, and without objecting at all to it we must not forget the importance of the era passed. We had at once two sects—two divisions. Some persons will say that these were not two sects properly, because they attended to different subjects; but probably no persons will say that they were not two sects in every sense but the name. There was a desire to separate from the religious question from a dislike to it, and this is already a sectarian element in society; no man can differ from society without being sectarian unless he is perfectly right, and when he is so we shall cease to give him any trifling name. But to separate from religious bodies because you adopt other religious opinions and make converts is to form a sect in the eyes of all; and to separate because you object to all religious opinions is equally to form a sect, unless you can show

thoroughly that you were absolutely right. Even then you become a division of society, and this division, begun in Gower Street formally, has spread over the land, so that we have the sect of physical science containing many men to whom the spiritual in religion has positively no meaning at all—being the fantastic creation of the brain. We do not say that all this is owing to Gower Street. It took a shape there, and did good. Now it is not here that we intend to give our opinion on the subject; we number both classes among our intimate friends, and we have our opinions; but at present we say that the movement was sectarian. Sadducees always have been reckoned as belonging to a religious sect, although denying that which to many men is the foundation of a religion. In other words, the study of merely physical science produces a class of men that influence the religious belief of a country and divide it.

Are we to decide which sect we shall belong to, and whether Oxford and Cambridge or the London University are to rule over the country? This would, in our opinion, be a backward step whichever we chose. Science is growing so rapidly that we cannot tell the limits to its power; we must give it free scope, we must allow its reasonings to have their full weight, and we must learn to give matter its full importance in our reasoning, seeing the great position it holds in creation.

It is our belief that these great representatives of human thought and progress, the universities, are essential to us in some form or other; it is difficult to tell what is the best form, but we may fairly decide which of the two classes of institutions is the widest or broadest. If we look at Oxford and Cambridge, or the universities in Scotland and Dublin, we find that, although beginning with teachings relating to the spiritual nature of man, they gradually have included more and more the physical.

The newer university, that of London, excludes the former. The other universities seem desirous to increase their professors in every direction; that of London excludes at least one direction. We therefore simply conclude that the older universities have a greater breadth; or look over more of the field traversed by man, and do not exclude either class of knowledge. They have certainly their opinions on one branch, but their studies comprehend both.

Be not surprised, therefore, if we look at the London University as sectarian in its views, and as fostering a sectarian knowledge. It must, in a sense, stand somewhat in the same position as the Methodist College or the

College of Independents, and partake of denominationalism. The result is in our opinion decidedly so. Men as a rule turn out to be what they are taught to be.

Are we therefore to blame the one or the other? Certainly not the old for being wide in theory. It will be said that this is only in theory, and that in practice their teaching is narrow, and that in former times it was still narrower, and therefore the London University was called into existence. This was partly true, and therefore we should be sorry to see it otherwise, at least for a while; this reasoning, however, simply shows that the new institution was supplementary, and did not even pretend to the greatest breadth. The spirit of this university seems to continue unchanged, and there is a growing tendency to the cultivation of science only. The exclusion of Greek from the necessities in the matriculated examination is a step in a similar direction, and one most resolutely fought for. The tendency is to the cultivation of the present. It is the same spirit that stimulates the manufacturer to despise science, and to make his son learn by apprenticeship the thumb-rules of his art, although for professions no university is so strict in encouraging true scientific principle. For this the nation owes it much.

Is it to the same feeling in the nation that we are to attribute the proposal of the premier to found a university which should not teach metaphysics? The occasion of the proposal would lead us to suppose that the cause was quite different; but the time at which it took place, and its associations, would lead us to think that he was moved by the spirit of the age, pressing in directions foreshadowed, but unseen except to a few.

This may be the case, and unknown even to the author himself, who certainly is not a man to be guided by merely material considerations. It comes at the time of the exclusion of Greek, and with the proposal of one examining board.

We are glad that there are the old and broad universities ready to receive all knowledge within them, lax sometimes in their rules, lax in their examinations on some points, but minute in others—like scholars careless in many things—but excessively careful of the points they study. The new comes out with business-like habits, numbers its students like workmen in a mill, knocks off those that are not up to the mark, and promotes the best, ruthlessly but successfully making good men of business—a magnificent manufactory of professional men—journeymen in science.

Should we like all England to have one examining board, all Scotland to have one, all Ireland to have one? Why not all the nation to have one? If we knew the truth in perfection, we should decide that all the world ought to have one, in sections, according to convenience of management. But we have not attained to perfect certainty in many things, and we object to have the sons of England educated as if we had. We must have a choice. If a man is narrow in his views, or if he desires that one young man shall have a professional education for teaching a sect, he sends him—let us say—to the Methodist College, or the Unitarian Hall, or the exclusively religious teaching of the Anglican Church, or the exclusively scientific teaching of the London University; but if we wish him to learn the struggles of humanity for knowledge, and the width and breadth of the attainment, we send him to a variety of classes, such as may be found at some of the older Universities, which are keeping up, or attempting to keep up, with modern times, and without bigotry are allowing the establishment of as many professorships as money can be found to maintain.

Yet there are men that would make the whole education of the country sectarian, that would destroy Oxford and Cambridge, as distinct units, and make one examining board decide the education of all the country. We have heard of a bed of Procrustes, but this is the most severe yet known to us; we have heard of inquisitions and faith-makers, and bigots, but none of them have ever set themselves up more decidedly above all their fellow-men than such a plan would exalt the proposed powerful organisation. Freedom of thought would, as a matter of course, be curbed. We should have only one educated sect, only one direction given to the general bearings of the mind, although the studies would be various. These great bearings decide that which we call character in individuals, so difficult to explain but so decided in its effects. These inexorable examiners, who are precluded from judging of any but intellectual feats and feats of the memory, would decide the mode of teaching and the things to be taught, and the still less exorable council would appoint the men to examine. The unhappy school teachers over all the country would be obliged to teach up to one standard, instead of, as now, having a choice; and instead of that variety of thought out of which new combinations are formed, one universal sameness would dominate in schools also, which would be as void of light

and shadow as the universities—dreary and dull. The evil is already showing itself, partially because as concentration goes on sameness increases; do not let us increase the evil. The man who can examine the young men of a college in metaphysics long enough to influence much of the habits of the teachers who send pupils to the colleges, and long enough to accustom them to his text books, has a power over the generation coming such as no other man has. A Prime Minister is nothing to him, and all the powers in Church and State must eventually yield more or less to his authority, although they may not know it. The examiner eventually directs the leading minds. In them we must consider real power to lie. It once lay in the army, it once lay in the song-makers, some one says; it lies to a great extent now with the reasoners, in all cases where they do not oppose the men of business, and the choice of modes of reason is with the examiners.

The genius of this nation has arisen in a great measure from the diversity of its population; this diversity has produced difference of training as well as difference of constitution. One great difference, that of training, would be removed by the one university alluded to, a system which has never been shown to produce good results.

It is not merely that there is no competition allowed by it, although that may be a loss, but there is no diversity; and there is no true freedom of thought where there is no diversity; and above all, there is in the exclusive character of the intention no sufficient breadth.

As proposed in Ireland, the narrowness was such as to reduce it merely to a school of certain branches.

It is greatly to be wished that no such schemes may be attempted in England, and it is equally desirable that no experiments of this lowering character will be brought into Scotland or Ireland, but that we should retain our universities founded on the structures laid not merely by the men of yesterday or the men of last century, but the great of all centuries, so that we may have institutions in which the wisdom and science of modern times, the devotion of mediæval ages, the strength of Rome, and the thoughtful searchings of Greece shall be side by side with the spiritual character and the search for holiness produced and hitherto producible only by the teachings of the East.

II. ACTINISM AND MAGNETISM.

By MUNGO PONTON, F.R.S.E.

HOW slow have mankind been in searching for and ascertaining the causes of physical phenomena! How tardy their efforts to apply their knowledge to practical purposes, even where the ultimate uses have proved to be of the highest importance to the well-being of the human race! How many ages had elapsed before Franklin discovered the cause of electrical phenomena—before Volta found how electricity might be developed by chemical action, and before Oersted perceived the mutual relations of electricity and magnetism! Even after the finger had thus been, as it were, pointed to a practical application, how many years intervened before this last discovery ripened into the construction of the electric telegraph!

In like manner, how many ages had elapsed before Scheele discovered the actinic action of light in blackening the chloride of silver; and what a number of years has it taken to develope that discovery into the art of photography!

It seems wonderful that the attention of mankind was not earlier attracted to the action of the sunbeams in developing or altering colours, and that they were not led to investigate the cause of this curious phenomenon. It might have been supposed that a careful study of Nature would have led them to perceive it to be the energy of solar light that tinges the cheek of the peach with crimson, gives the apricot its flesh-like tint, imparts to the harebell its beautiful blue, paints the pansy with alternating brilliant yellow and deep violet, reddens the rose, and dyes the tulip with its richly varied hues. The first attempts at tracing the operation of the sunbeams in the colouring of flowers were made by screening the petals from the action of the light; but these experiments went no farther than to show that, in some cases, the petals do not acquire their proper hue when they are thus screened. The subject, indeed, was little studied until after the discovery of the actinic action of solar light on other substances. Even yet, one of the most remarkable cases of what may be termed natural photography is but little known. It is that of the beautiful bell-flower of the *Cobæa scandens*, which on the first day of its opening is of a pale-greenish white, but after exposure for two or three days to the actinism of solar light acquires

a rich purple. This actinic action on the juices of plants has not been deeply investigated, nor has it as yet been applied to any practical purpose.

Another example occurs in the animal kingdom. The common earwig, if reared in the dark, is almost colourless, being of a nearly uniform creamy white; but if it be subsequently exposed for some hours to moderate daylight, it will eventually acquire its natural dark colours.

It appears to be part of the wise dispensation of Divine Providence in the government of the human race, that the most useful discoveries should be made only after the exertion of a great amount of industry, applied with much wisdom and skill—and that, too, not by a single individual, but by a long succession of men. It is given to one to discover a principle, to another to take advantage of it for the attainment of some practical end, to a third, a fourth, and a fifth to make successive improvements in the working out of the principle, and in modifying its mode of action. Thus, Niepce first discovered the effects of light on films of bitumen; this result suggested to Daguèrre the application of iodine vapour to produce on plates of silver a film sensitive to light, and the subsequent development of the image by mercurial vapour—a photographic process which bears the name of its inventor, It was Scheele's discovery of the action of light on chloride of silver, followed up by Wollaston, that led Fox Talbot to its practical application in obtaining photographic images on paper, and to his further discovery of the mode of producing a latent photographic image on iodide of silver capable of subsequent development by the application of a powerful deoxidising agent. These results paved the way for Archer, who availed himself of Schœnbein's discovery of soluble cotton or collodion to spread a film of that substance on glass, and charge it with iodide of silver, so obtaining a more sensitive and manageable medium for the reception of the latent photographic image, to be afterwards subjected to the action of a developing agent. Other and later labourers in the field have greatly improved on those earlier methods, until the taking of pictures by means of salts of silver and developers has now reached a pitch of perfection of which the earliest pioneers in the art had scarcely dared to dream.

To the lot of the author it fell to discover the photographic properties of the double salts of chromic acid when in contact with organic matter, and the curious fact that the disengagement of the chromic acid from the salt under the action of light, and its immediate re-combination with

the organic matter, operates in the latter a great change, rendering gelatine, albumen, and such like substances insoluble. It was reserved, however, for a succession of other labourers in the field to develope this discovery into the method of printing photographs in gelatine, impregnated with carbon and other pigments. Under the action of light, gelatine, charged with the bichromate of potash or ammonia, becomes insoluble by warm water in exact proportion to the degree in which it has been affected by the light. Hence, by spreading gelatine in plates of some degree of thickness on films of collodion, exposing these with their collodion side next the negative, and subsequently dissolving away the portions more or less unaffected by the light, pictures are obtained in relief. Of this property Woodbury availed himself to take metallic casts from those pictures in relief, and from these metallic plates to take impressions on paper in pigmented gelatine.

The utilisation of the original discovery has been recently brought to still higher perfection by its having been found that the plates of gelatine, thus impressed by light, may themselves be rendered directly available for obtaining impressions on paper in engraver's ink. Yet how simple the matter appears now that it is known! When on a plate of glass, previously coated with white wax dissolved in ether, there is spread a plate of gelatine charged with bichromate of potash and chrome alum, and when, after being allowed thoroughly to dry in the dark, this gelatine plate is removed from the glass, and placed under a negative photograph, wherever the light penetrates, the gelatine becomes, in a greater or less degree, not only insoluble in warm water, but incapable of imbibing moisture. But the parts thus acted on by light can, with greater or less degrees of readiness, receive engravers' ink. Those portions which have been most hardened by the light will receive the stiffest ink; those which have been but partially hardened will take on the ink only when it is more or less diluted; while those portions which have escaped the action of the light, and have become moist (but only very slightly swollen) from imbibing water, refuse the ink altogether. In this manner every gradation of shade may be given to the impression produced from the gelatine plate, and it is said that as many as 1500 impressions may be taken from the same plate, direct pressure being employed. This last, which is the most perfect application of the double salts of chromic acid to photographic purposes, is due to the laborious industry and skill of Ernest Edwards.

The two last-mentioned processes, in both of which the copies are multiplied by purely mechanical means, afford the most expeditious and economical methods of attaining that end. The copies thus produced, however, are not strictly speaking photographs; while, to an artistic eye, they are inferior in delicacy to those obtained from the primary negative by direct actinic action. Much skill has accordingly been directed towards perfecting the processes by which the latter sort of pictures may be produced. It was first pointed out by Mr. Blair that the best mode of bringing the actinism to exert its effect on the pigmented gelatine, is to make the light act from behind, so as to allow its hardening influence on the gelatine to penetrate to different depths, according to the lights and shades of the negative. Hence arose the practice of taking the impressions first on paper coated with pigmented gelatine, and thereafter transferring it to white paper coated with simple gelatine. Very good effects were obtained in this way, but the pictures laboured under the disadvantage of presenting the image reversed as respects right and left.

To rectify this reversal, recourse was had to the method of double transfer, as practised by the Autotype Company. In this process the picture is first transferred from the black bichromated gelatinised paper to a plate of zinc, and when the picture has been fully developed by washing the plate with luke-warm water, it is transferred from the zinc to white gelatinised paper, on which it appears rectified in position. An improvement on this method was subsequently effected by Mr. Johnston, of the Autotype Company, who discovered that, by coating white gelatinised paper with a film of wax and grease in certain proportions, the image, if first transferred to this paper, may be re-transferred from it to another piece of white paper, prepared with a strong solution of simple gelatine. By this plan, not only is the picture rectified in position, but the pigment, by imbibing a small portion of the wax and grease, becomes assimilated to engravers' ink, and adheres firmly to the paper. The brilliancy of the picture is increased by washing it with benzine. For the use of amateurs this last mode of printing in carbon is the best as yet devised, and it reflects great credit on the skill of its inventor.

In copying portraits, the author has obtained peculiar and striking effects by the following method. The portrait should for this purpose be taken with a dark background—that of the negative being nearly, though not quite, transparent. The bichromated black gelatinised paper is to be

exposed under the negative for three or four times the period required for an ordinary picture. A plate of glass, thoroughly cleansed, having been gently warmed, receives a thin equable coating of Sœhnée varnish. When this is dry, the picture is transferred from the black gelatinised paper to the glass plate, under luke-warm water in the usual manner. The picture is then to be washed clean, and allowed to dry thoroughly. A margin of very thin paper having been applied all round it, a second very clean thicker glass plate is to be laid over the picture, and carefully cemented to it all round the edges. The picture is thus enclosed between the two plates. The back glass (the thicker of the two) must then be coated with Brunswick black all over the background of the picture, the outlines of which must be carefully traced, so that no light may penetrate between the picture and the background. When the black varnish is quite dry, the picture is to be placed at an angle of 45 degrees, with a piece of mat gilt paper below it. When the transparency is thus viewed by the light reflected from the gilt paper, it presents the appearance of a bas-relief. This effect is so decided, that the spectator can hardly persuade himself that he is looking on a flat surface.

While the attention of investigators has thus been for a considerable number of years past directed almost exclusively to ascertaining the best means of rendering the actinic properties of light available for practical purposes, and rightly so, it is not well that the theoretical questions connected with actinism should be entirely neglected; for a thorough search into the principles and modes of actinic action is the most promising way of arriving at results which may eventually prove of further practical utility.

One of the earliest and most interesting questions which presented itself to the inquiring mind, was the possibility of explaining actinic action in accordance with the principles of the undulatory theory of light. In a former work by the author, the first edition of which was entitled "The Material Universe," and the second was (much against his wish) entitled "The Great Architect," he indicated the manner in which the formation and subsequent development of the latent photographic image, both in the process of Daguèrre and in that of Talbot, might be explained agreeably to the undulatory theory. A further development of his views was subsequently published in "The Engineer," and again briefly re-stated in the notes of his more recent work, entitled "The Beginning, &c." But as his ideas have thus

been brought before the public in rather a piecemeal sort of way, it may not be deemed amiss that they should be here presented in a more regular and condensed form.

The duality observed in all electrical and magnetical phenomena, whether paramagnetic or diamagnetic, raises a strong presumption that there is in nature a *somewhat* to which this dualism is due. The remarkable circumstance that the magnetic influence, with its duality of manifestation, passes through the free ether, renders almost compulsory the inference that it is in this subtle medium that the origin of the dualism is to be sought. It is well known that there is a remarkable connection between the earth's magnetism and the solar spots, and that all magnetic observatories in our globe have been affected by certain sudden luminous flashes which have been observed in the solar photosphere. There can, therefore, be no doubt of the fact that the magnetic influence does pass through the free ether, and that magnetic dualism is thus wafted onwards like luminous waves through the ethereal expanse. Now it is almost inconceivable that this should happen unless there were some intrinsic dualism in the ether itself—unless, in short, it were composed of two fluids, which, like the nitrogen and oxygen gases of our atmosphere, are mechanically alike, but chemically different. It is needful to suppose them to be mechanically alike—both perfectly elastic fluids, and that, on their particles being set a vibrating, they vibrate in the same times, and that these vibrations are wafted onwards in similar waves of definite lengths. At least, there have not yet been distinguished any phenomena from which it could be inferred that the supposed two fluids differ from each other in their mechanical constitution in any appreciable degree. But it is equally needful to suppose these two fluids to differ from each other in their relations to the molecules, ultimates, and atoms of bodies endowed with the energy of gravitation, of which the ethereal fluids are themselves destitute; for it is only by such differences that the existence of two ethereal fluids can be established.

Waiving for the present the question of the relation of the duality of the ether to that of magnetism, and regarding meanwhile the former as merely a convenient assumption, let us, by means of it, endeavour to explain actinic action in the case of iodide of silver, and in particular to account for the formation of the latent image, and its subsequent development.

Let it be granted that each molecule of the iodide of

silver consists of an ultimate of silver, retaining by a strong attraction, in close proximity to itself, an ultimate of iodine, there being, however, at their nearest points, a minute space filled with the luminiferous ether in a highly compressed condition. Let it be further assumed that, in this interval, the two ethereal fluids subsist in a state of partial separation —the one being accumulated next the silver ultimate by reason of its being less repellent towards silver, the other next the iodine ultimate by reason of its being less repellent towards iodine. Call the former of the two ethereal fluids parargyrine, and the latter pariodine. Suppose that, in the system of luminous waves in a beam of light approaching this molecule, there are certain of the waves whose vibrations are synchronous with those which the silver ultimate tends to assume, and certain others whose vibrations are synchronous with those which the iodine ultimate performs in its tremors. The result will be that the silver ultimate will begin to vibrate against the iodine ultimate, and the two will alternately approach and retire. This, however, they cannot do without promoting a re-admixture of the parargyrine with the pariodine in their normal proportions. Such a re-admixture again cannot take place without weakening the attraction between the silver and iodine ultimates; for the silver ultimate begins to be urged by the repulsive energy of the pariodine, which is for it greater than is that of the parargyrine; while the iodine ultimate becomes exposed to more of the repulsive energy of the parargyrine, which is for it greater than is that of the pariodine. The consequence will be that, at the moment when by the vibration the ultimates of silver and iodine are farthest apart, the weakening of the attraction between them will be considerable. If now there be introduced another chemical molecule or ultimate having a strong attraction for the iodine, the probabilities are great that, at the moment when the vibration attains its extreme amplitude, the iodine will permanently leave the silver, and attach itself to the introduced molecule or ultimate for which it has, at that particular moment, a more powerful attraction.

On this general principle may be explained the development of the latent image, both in the case of the Daguèrreotype, and on that of the collodion film. In either case it is needful to assume that the vibrations established by the action of the incident light continue for a considerable time after exposure.

In the Daguèrreotype, the effect of the light appears to

be the disengagement of the iodine from the ultimates of silver near the surface, where the vibratory action is greatest—so allowing it to penetrate inwards and attach itself to those silver ultimates, which are less agitated by the motion. The iodine thus, as it were, eats its way inward, leaving behind it ultimates of silver more or less disengaged. Accordingly, when the plate is exposed to the action of mercurial vapour, the ultimates of mercury attach themselves to those disengaged ultimates of silver, forming with them a white amalgam, which constitutes the lights of the picture—the unamalgamated parts of the silver forming the shadows; while the unaltered film of iodide of silver is removed by the hyposulphite of soda.

The latent image in the collodion processes presents two cases—the one that of the wet film, the other that of the dry. The case of the wet film resembles that of the Daguèrreotype. The silver ultimate and the iodine ultimate, which it retains near it by its attraction, begin to vibrate under the stimulus of the incident light. The parargyrine and pariodine in the intervening space are thus forced to intermingle—so weakening the attraction between the two ultimates. This condition continues for a considerable time after the stimulus of the external light is withdrawn. When a developer is applied, it takes advantage of the moment of greatest weakness, when the ultimates of silver and iodine are in the course of their vibration farthest asunder, and it effects their separation, the iodine combining with the developer, while the silver resumes the metallic form. The deposit of silver in this case constitutes the shadows of the picture when it is viewed as a transparency, and it is then accordingly negative; but when the picture is viewed by reflected light with a piece of black velvet behind it, the deposit of silver forms the lights of the picture which is then positive, and such are some of the most pleasing photographs.

When the collodion film is dry, again, it requires more applied energy to establish the vibratory condition as between the silver and the iodine ultimates, owing to the rigidity of the film; and for the same reason the vibrations are probably arrested the moment that the stimulus of the external light is withdrawn. But the attraction of the silver for the iodine has been permanently weakened through the action of the light, by reason of the re-admixture of the parargyrine with the pariodine, in the interval between them, resulting from the vibrations. Hence, at the moment of the arrest of motion, the ulti-

mates of silver and iodine are farther asunder, in the case of those which have been exposed to the light, than they are in the case of those which have not been thus stimulated into vibration. Accordingly, when a developer is applied, even a very long time after exposure, decomposition ensues and silver is precipitated.

When ozone is applied after exposure and before a developer, it prevents the action of the latter. For the attraction of ozone for silver is powerful, and uniting itself to that of the iodine, it prevents the silver from being reduced to the metallic condition by the developer. Or perhaps it may neutralise the action of the latter, by supplying it with something which it prefers to iodine. The vapours of chlorine, bromine, fluorine, or iodine, applied after exposure to the light, would probably in like manner prevent the action of the developer.

On the other hand, it is well known to photographers, that the presence of a small quantity of free nitric acid greatly helps the action of light on the iodides, chlorides, and bromides of silver. It is not difficult to discover the part which this free nitrate performs. Confining attention to the case of the iodide of silver, with a slight admixture of the nitrate applied to the collodion film, and exposed while moist to the action of light, we must suppose that, while the silver and iodine ultimates vibrate against each other, a similar vibratory condition is established as between the ultimate of silver, and the molecule of nitric acid with its combined molecule of water. Now in the agitated condition of these substances, the iodine may momentarily be brought more within the attractive influence of the constituents of the nitric acid than of the ultimate of silver, and may form temporary unions with those constituents—hydriodic and iodic acids, and iodide of nitrogen in small quantities; while a portion of the oxygen of the nitric acid may temporarily become more intimately engaged with the ultimates of silver. If a deoxidising agent be applied while this state of affairs subsists, the oxygen will be easily disengaged from the metallic silver—the iodine becoming otherwise permanently occupied. In the case of the dry collodion film, again, the interchange of the iodine and the oxygen may become more permanent; so that at whatever distance of time the developer be applied, it has to withdraw only oxygen, not iodine, from the silver. The cessation of the action of the developer, however, after a short interval, in the case of the moist film, seems to indicate that, when the vibratory action excited by the light ceases,

the oxygen and iodine resume their original positions and relations, so that all things return to the same condition in which they were before the film was subjected to the actinism of the light.

When chloride of silver is applied to paper, in conjunction with a little free nitrate for printing purposes, the nitrate greatly quickens the decomposition of the chloride. This acceleration can hardly be explained on any other supposition than that the agitation, established by the actinism, enables the chlorine to migrate from the silver to the constituents of the nitric acid—forming hydrochloric and chloric acids—perhaps also chloride of nitrogen, while part of the oxygen of the acid attaches itself to the silver. Hence the colour produced is no longer the purple of the pure chloride, but there is a large admixture of brown from the oxide.

In the case of the double salts of chromic acid, the separation of one of the molecules of chromic acid from the base takes place by the action of the incident light alone, without the aid of any developer, other than the organic matter to which the double salt has been applied. It results from the vibratory condition established in the salt by the actinism, which, as it were, shakes free one of the molecules of chromic acid, and allows it to combine with the organic matter. There is here no latent image properly so called; nevertheless in the case of black pigmented gelatine the image is, owing to its blackness, invisible. But it may always be rendered visible by immersing the picture, after exposure, in cold water for an hour or so, when the picture will be seen standing out in relief. The portions of the gelatine, which have not been acted on by the light, swell through imbibing the moisture, and that in exact proportion to the degree in which they were protected from the light's actinism. Those parts which have, by the action of the light, been made to combine with the chromic acid, cease to have the power of imbibing moisture and swelling under its influence.

Some of the metals, more especially zinc, silver, and magnesium, when used as electrodes, generate ethereal waves lying far beyond the limits of the visible spectrum, yet capable of exerting a powerful actinic action. Thus an object might be photographed by means of actinic waves wholly invisible to the eye. This circumstance tends to establish the supposition already propounded, that the vibrations set up by actinism must be very minute, and such as are likely to take place between the ultimate of

silver and the ultimate of iodine, rather than such as might be expected in the movements of the entire molecules of iodide of silver.

It will be perceived that it is only the latent image in the case of the iodised films, either in the Daguèrreotype or collodion processes, that requires for its explanation the help of the assumption, that the ether consists of two perfectly elastic fluids intimately mingled together, yet capable of partial temporary separation. Should this same assumption be eventually found available for explaining the duality observed in the phenomena of electricity, paramagnetism and diamagnetism, the evidence in favour of the hypothesis will be greatly strengthened by the circumstance of its being thus found to render such aid in explaining a phenomenon so diverse from these as the latent image.

The facts that metallic zinc, when thrown into a state of heated vapour, generates in the ether waves which, though wholly invisible, yet exert much actinism, and that aluminium, under similar circumstances, originates invisible waves capable of exciting fluorescence, favour the idea that the influence of the solar radiation on the magnetic needle may also be due to waves which are in like manner invisible. Experiments have been several times made with the view of showing that the violet and ultra violet waves do affect the magnetic needle; but the results, probably owing to the difficulties attending the experiment, have not been decisive. It would not be easy, however, to account for the known effect of solar radiation on magnets —more especially the effect of solar spots—otherwise than by supposing that there are special waves of some sort that pass through the ether, and either excite or alter the magnetic condition. If such be the case, then will it be probable that, exactly as all bodies which receive an accession of temperature from the solar energy give it off again by radiation, even so all magnetic bodies receiving an accession of magnetism from the solar energies give it off again in a similar manner, propagating from themselves back waves having a great rapidity of vibration — too great to be appreciable by the optic nerve, but nevertheless capable of exciting or maintaining either paramagnetism or diamagnetism in other bodies. Nor does it seem to be improbable that, as in the case of fluorescence, in which there is a change in the rate of vibration operated by the action of the ponderable particles, there may be, in the case of the magnet, something similar. Magnetic bodies may have

their particles thrown into the state of magnetic or dualistic vibration by ethereal waves, which in other bodies would produce quite different effects, and may reciprocally produce in the ether back waves having the same dualistic properties.

The supposition that the ether consists of two fluids might throw much light on the magnetic condition. For it might be explained by supposing the fluids to become separated from each other in the pores of magnetic bodies to a much greater extent than in the case of other bodies—the separation taking place lengthwise in the case of paramagnetic bodies, and crosswise in the case of diamagnetic bodies, so that in the former all the atoms having an atmosphere of parargyrine are turned towards one end, and all those having an atmosphere of pariodine are turned towards the opposite end; whereas, in diamagnetic bodies, a similar arrangement subsists laterally. Any waves having their origin in ether, whose constituent fluids might be thus separated, would have a direct tendency to become double-sided—that is to say, in such a wave the particles displaced towards the one side of the line of propagation might at any given moment be all of parargyrine, while those displaced towards the opposite side of the line of propagation might at the same instant be all of pariodine. Nor does it appear impossible that, in a similar manner, there might be generated invisible magnetic waves, which should be polarised in opposite planes—those whose vibrations are performed in one plane affecting only the parargyrine: while those whose vibrations are performed in the opposite plane affect only the pariodine. The double-sidedness of the waves, however, seems to be a more probable explanation of dualism, as it subsists while passing through the free ether.

That the condition of dualism is actually transmitted through that medium, the connection between the solar spots and terrestrial magnetism seems to render it almost necessary to conclude. Were it not for that connexion, it might be enough to suppose that, in magnetic bodies, the tendency to a separation of the two ethereal fluids is favoured and augmented by a vibratory condition of the particles of the magnet; while in the case of the silver salts, the already existing partial separation of the two fluids in the interval between the silver ultimate and the other ultimate with which it is in combination, is neutralised by the vibratory motion. In the latter case, the vibrating ultimates are supposed to be very near each

other, and by their oscillations mechanically to mix the two fluids. But in the case of a magnet, the vibrating particles are farther apart, and the effect of the vibration seems to be to drive away the intervening parargyrine from one of the kinds of atoms of which the ultimate of iron consists, and the pariodine from another set of those atoms—so promoting the separation of the fluids.

The permanent condition of magnetism does not appear to be capable of explanation, except on the supposition that every such magnet has the power to convert invisible ethereal waves into magnetic waves. There is even in the dark a constant interchange of radiation between magnets and all other surrounding bodies. Now if magnetic, like fluorescent bodies, have power to alter the rate and character of vibrations communicated to their particles, it is not difficult to imagine, that what comes to the magnet as radiant heat, or light, or actinism, may in part at least be given off again as radiant magnetism with its concomitant dualism, and that what in other bodies serves only to maintain their temperature at a pitch corresponding to that of all surrounding bodies, serves in the case of a magnet partly to maintain its magnetism at a certain rate of tension.

Magnetism is quite as much an energy as temperature; nor does it appear more possible for a magnet to maintain its magnetism without a continuous fresh supply of motive energy, than for a body to maintain its temperature without a like supply. Moreover, as the motive energy of temperature is undoubtedly capable of being converted into the motive energy of magnetism, and *vice versâ*, it seems no more than reasonable to suppose this conversion to be in continual progress in the pores of a permanent magnet. Thus it appears unnecessary to look farther for the needful supply of motive energy which maintains permanent magnetism, than primarily to the ethereal waves transmitted from the sun, and secondarily to the continuous radiation emanating from all surrounding bodies. Doubtless a portion of this energy goes to maintain the temperature of the magnet; but it must be borne in mind that this temperature is nothing else than a certain amount of vibratory motion in the particles; nor does it appear improbable that such a vibratory condition cannot subsist, without tending to uphold the magnetic state, where it has been already developed. Such a degree of cold as would reduce this vibratory condition to a very low point would no doubt destroy, or at least suspend, the magnetism of a permanent magnet.

III. MAGNETO-ELECTRIC ILLUMINATION.

By WILLIAM CROOKES, F.R.S., &c.

THE progress made in electric illumination during its advance towards perfection has been several times recorded in the pages of this journal. In our first number, published nearly ten years ago, Dr. J. H. Gladstone gave a history of the early difficulties attending the introduction of the magneto-electric machine as a light-generator for lighthouse illumination. Two years subsequently, the present writer described Wilde's magneto-electric machine, and, after a further lapse of years, during which time no very important improvement in the industrial application of magneto-electricity has been recorded, another step in advance has been made which calls for detailed notice.

The chief difficulties in the employment of magneto-electric currents for industrial purposes have been their almost instantaneous character and the rapid alternation in their direction. The instrumental means necessary to seize hold of these rapidly alternating waves, and convert them into a more or less continuous stream of force flowing in one direction, are necessarily of a delicate character, and are easily put out of adjustment. This is easily understood when it is remembered that in the machine first tried by Mr. Holmes the rubbing surfaces were worn away in ten or twenty minutes. The Berlioz machine required for its maximum of intensity 350 or 400 revolutions per minute, and the direction of the current is then reversed nearly 6000 times per minute; here, however, the alternate currents are not brought into one.* In the machine made by Mr. Wilde for the Commissioners of Northern Lighthouses, the first armature is made to revolve about 2500 times a minute, generating 5000 waves of electricity. These alternate currents are converted into an intermittent current moving in one direction only by means of a commutator. The second armature revolves 1800 times a minute, generating 3600 alternately opposed waves of electric force, which are picked up and sent in one direction by a commutator, as in the former case.†

It is evident that when a good friction contact is to be kept between pieces of metal moving at these enormous velocities, the wear and tear is very great. For a long time,

* Quarterly Journal of Science, vol. i., p. 73. January, 1864.
† *Ibid.*, vol. iii., p. 504. October, 1866.

however, it was thought that these difficulties were inherent to the magneto-electric machine, until electricians found, first, that the almost instantaneous flash of the current could be considerably lengthened out, and then that the successive waves generated could be so produced as to flow in the same instead of in opposite directions.

These important desiderata are supplied in a magneto-electric machine of a novel form, invented by M. Gramme. The principle is not difficult to understand. Take a long bar of soft iron, E, E′, Fig. 1, round which is coiled an insulated copper wire; to this bar, forming an electro-magnet, let a permanent magnet, S N, be presented, the south pole being nearest to the iron bar. Now move the permanent magnet in the direction of the arrow parallel with itself, with a uniform velocity, and always maintaining the same distance from the bar. The south pole of the permanent magnet will produce a north magnetic pole in the portion of the iron bar nearest to it; and the gradual displacement of this pole from one end to the other of the iron bar, caused by the motion of the magnet, will induce in the surrounding wire an electric current which may be rendered evident by the galvanometer, G. This current will not be instantaneous: it will continue to flow during the whole time the magnet is moving between the two ends, E E′, of the iron bar, and its time of duration may therefore be varied at pleasure.

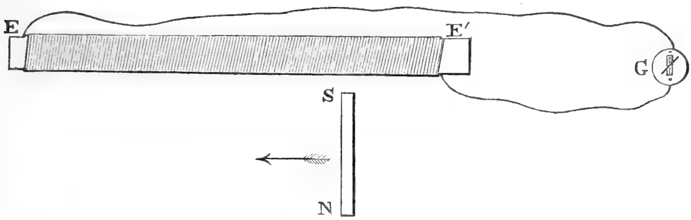
This experiment shows that it may be possible, by proper arrangements, to realise a machine which will furnish a continuous current of electricity for as long as may be desired. We have only to imagine the electro-magnet, instead of being the straight bar shown in Fig. 1, bent into a circular form as at E, E′, E″, E‴, Fig. 2.

Submit this annular electro-magnet simultaneously to the influence of the two poles of the permanent horse-shoe magnet, N S, and at the same time imagine it to revolve on its axis in the direction shown by the arrows.

The south pole, S, of the horse-shoe magnet will produce in that portion of the ring, E, which is near it an electric current in a particular direction, as may be inferred from what we have said respecting the straight bar, Fig. 1. But the north pole, N, of the magnet will likewise produce in the part of the ring which is in its neighbourhood, E″, an electric current flowing in the opposite direction; and it is easily conceived that in the two portions of the ring, E′ and E‴, which are in what may be called the mean position, there is no current at all. If, therefore, we wish to collect

the two contrary currents produced simultaneously in the wire surrounding the electro-magnet, we have only to connect the wires at the mean position to two conductors by friction contacts, F F′, when the current can be carried away to a galvanometer, G, and rendered sensible.

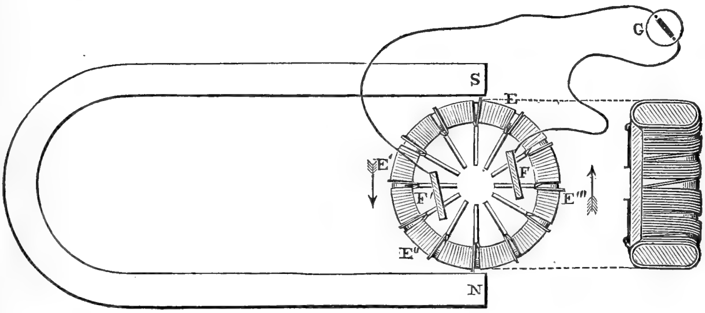
FIG. 1.



The principle of the arrangement being thus understood, the construction of the machine itself will be readily intelligible.

It consists of a permanent horse-shoe magnet, S, O, N, Fig. 3, between the poles of which revolves an electro-magnet. This electro-magnet consists of a ring of soft iron, round which is wound an insulated conducting wire, presenting no solution of continuity. It may be conceived as being an ordinary straight electro-magnet bent round in

FIG. 2.



a circle, and the two ends of the conducting wire soldered together to establish continuity.

In Figs. 4 and 7 the electro-magnet is represented at A in section, whilst in Figs. 3 and 5 it is shown at A with the covering wire on it. It revolves round its axis on an axle to which movement is communicated either by means of

belting or with toothed gearing, shown in Figs. 3 and 4, worked by a handle, M.

The current is generated and collected in the following way:—The wire surrounding the electro-magnet is, as we have said, continuous, but it is disposed in 40 sections or elements, each consisting, say, of 100 turns. The outer end of the coil of one section forms the commencement of the first coil of the next section, and so on. The whole of the wire is therefore divided into 40 equal sections, being, however, continuous throughout.

To understand better how an uninterrupted current is produced, let us imagine a line to be drawn equatorially, or perpendicular to the lines of force between the poles of the horseshoe magnet, and dividing the ring armature into two parts; suppose likewise that to the two ends of one of the 40 coils two wires are soldered, the other ends of which are attached to a galvanometer. Now let the ring be intermittently revolved in one direction, so as to give to the said coil a succession of movements of about 10 degrees, stopping each time to permit the galvanometer needle to resume its normal position. It will then be seen that the whole time the coil is above the equatorial line the galvanometer needle will be urged in the same direction, and the currents may be called *positive*. But as soon as the said coil crosses the equatorial position, the currents generated in it will be *negative*, and in the opposite direction to what they were at the other half of the circle. This experiment shows that a reversal of the direction of movement carries with it a reversal of the direction of the current.

From this insight into what is produced in one of the sections, the general phenomena produced by the whole circle of coils are easily understood. The 20 sections which are on one side of the equatorial position are the source of positive currents; these may be of unequal intensity among themselves, but for a uniform velocity of rotation their sum is evidently constant, for as one coil crosses the equatorial line from north to south an opposite one comes up from south to north to take its place. On the other hand, the 20 sections which are on the other side of the equatorial line are the seat of negative currents, the sum of whose intensities is likewise constant, and equal to that of the positive currents.

Thus the revolving armature presents two groups of coils, generating two equal but opposite streams of electric force. The wire being unbroken the currents neutralise each other,

and there is no circulation. The result may be likened to what would be produced by taking two batteries, each of 20 cells, and connecting them in opposition by joining similar poles.

The problem now is to pick up these dormant currents and utilise their force. Its solution is apparent from the comparison we have just made. To collect the electric current from two batteries which are connected together in opposition, it is only necessary to fasten conducting wires to the two points of contact of similar poles, when the whole force of the batteries will flow along these wires. They were hitherto opposed, they now flow together, quantity-wise. M. Gramme, in the second portion of his invention, has adopted this artifice in an ingenious manner.

The various sections of the continuous electro-magnet are connected with radial pieces of copper shown at R in Figs. 3, 4, and 7, insulated one from the other, but coming very close. The termination of one coil of wire and the commencement of the adjacent coil are soldered to the same radial connector, of which therefore there are as many as there are coils. These radial connectors, on approaching the centre, are bent at right angles, as shown at R, Figs. 4 and 7, and pass through to the other side, where their ends form an inner concentric circle, being still insulated one from the other.

Two friction pieces F (Figs 4, 5, and 6) consisting of discs of copper, are pressed by means of springs shown at *r* (Figs. 5 and 6) against the circle formed by the extremities of the conducting radii R, at two points which are accurately in the equatorial line; that is to say, at the place where the equal and opposed currents generated in the upper and lower halves of the ring neutralise each other. Consequently the currents are collected and flow together along conducting wires, which are fastened to the friction pieces F.

The perfect continuity of the current so obtained, is secured by causing the friction pieces F to touch simultaneously several of the radial conductors R; consequently the metallic circuit is never broken.

The effects produced by these machines vary with the rapidity of rotation. Experience shows that the electromotive force is sensibly in proportion to the velocity; but it is probable that this force tends towards a limit, corresponding to a particular velocity, beyond which the electromotive force would remain constant, or even diminish. Moreover, the electromotive force is greater in proportion to the

number of coils encircling the iron ring, but the relation between these two quantities has not yet been determined. The theoretical resistance of the machine should be one-fourth of the whole resistance of the wire wound round the ring armature; but the actual resistance is not so great, since each friction-disc always touches several radii, R, and the resistance of the coils thus embraced by the friction-disc has to be subtracted from the resistance of the circuit.

The possibility of augmenting the strength of the current by increasing the dimensions of the machine is too obvious to need more than a passing allusion. The effects may also be increased by connecting together several such machines, as galvanic piles are connected, either for intensity or quantity. The quality of the current likewise differs according to the kind of wire surrounding the armature, a short thick wire producing effects of quantity, and a long thin wire, of intensity. It is also easy to see that two horse-shoe magnets, instead of one, may be made to act on one ring armature; that is to say, it may be actuated by four poles instead of two, or even by a greater number; always having a friction-disc between each pair of poles. Moreover, the permanent horse-shoe magnet may be replaced by electro-magnets, which can be excited by a portion of the current derived from the machine itself, according to the now well-known method. At the beginning of rotation the residual magnetism of these electro-magnets will induce a feeble current in the ring; one half of this passes round the electro-magnets, the four poles of which react on the armature. Of the four friction pieces, two carry half the current to excite the electro-magnets, and the machine rapidly attains the maximum effect. From conducting wires attached to the other two friction pieces a powerful current is available.

A machine of this kind, containing two horse-shoe electro-magnets, one for exciting and the other for the exterior current, and having round each pole 7 kilos. of copper wire 3 m.m. diameter, when worked by hand, decomposes water, and fuses 26 centims. of iron wire 9-10ths m.m. in diameter. However slowly the armature is rotated, the needle of a large galvanometer having the wire only once round is deflected, and the effects increase in intensity as the velocity of rotation increases, up to a maximum of 700 or 800 turns a minute, a velocity which is easily obtained when steam is employed.

Such a machine, giving an absolutely continuous current

Fig. 3.

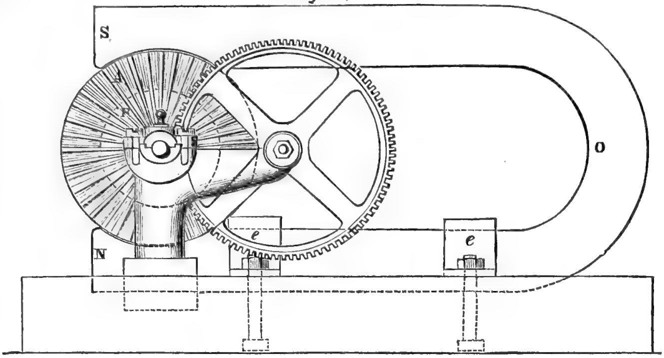


Fig. 4.

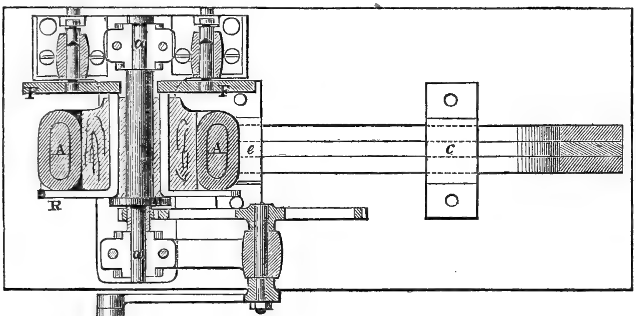


Fig. 6.

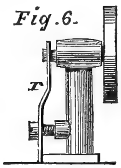


Fig. 5.

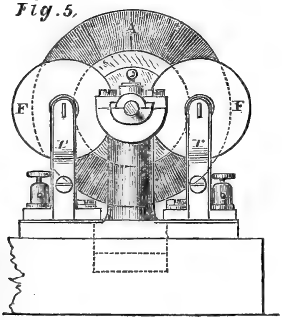
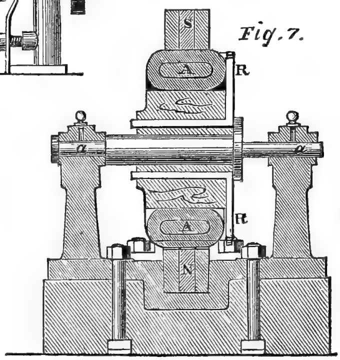


Fig. 7.



of electric force by the mere turning of a wheel, is of value outside the physical laboratory. It is available—(1) for medical purposes; (2) for telegraphy; (3) for electro-plating, gilding, &c.; (4) for military purposes, signalling, explosions, &c.; (5) for chemical decompositions; and (6) for electric illumination.

A large machine, which has lately been exhibited in London, driven by a $2\frac{1}{2}$-horse-power engine, produced a light equal to 8000 candles; a copper wire about $1\frac{1}{4}$ m.m. in thickness, suspended between the poles, became instantly red-hot with a revolution of little over 300 in a minute. Larger machines are being made that will probably give a light equal to 25,000 candles.

This machine has lately been examined by the French *Société d'Encouragement*, and in accordance with the recommendation of the reporter, Count du Moncel, a prize of 3000 francs has been awarded for it to M. Gramme; whilst the manager of the "Alliance Company," M. Joseph Van Malderen, who superintended its manufacture, has had awarded to him a gold medal. In his report, Count du Moncel says that a machine 1·25 metre in height, 0·8 metre long, and the same in width, driven by a 4-horse engine, gave a light equal to 900 carcel lamps. It also heated to redness two juxta-posed copper wires 12 metres long and 0·7 m.m. diameter, and fused an iron wire 2·5 metres long and 1·3 m.m. thick.

The constancy of direction of the electric current generated by this machine is, however, not of so great an importance for the electric light as for other purposes for which it may be used. Indeed, the electric light is by many electricians thought to be superior when produced by a magneto-electric machine of the old form without any commutator. The alternate reversal of the currents of electricity produces no flickering or irregularity in the arc of light, as they occur far too quickly to be appreciated by the eye, whilst the rapid reversal of the direction causes the carbons to wear away with great regularity, thus enabling the point of light to be kept more easily in the focus.

For the electro-deposition of metals—copper, silver, &c., constancy of direction of current is indispensable, and here the experiments show a marked superiority of the Gramme machine over other magneto-electric machines.

In the galvanoplastic works of M. Christofle, of Paris, where experiments have been going on for more than a year, it is found that the best machine hitherto known, when moved with a velocity of 2400 revolutions per minute, only deposits

170 grammes of silver per hour; whilst a smaller Gramme machine moved with a velocity of 300 revolutions per minute deposits 200 grammes of silver per hour; the temperature of the annular armature not exceeding 50° C., with a velocity of 275 revolutions, no elevation of temperature is experienced. It will be easily comprehended how strongly this result, obtained with a speed of rotation eight times less than hitherto required, speaks in favour of M. Gramme's invention. Usually at M. Christofle's the circuits are arranged to deposit 600 grammes of silver per hour, and the manager of the factory finds that the deposition with this machine takes place with a regularity and constancy which leaves nothing to be desired, and which cannot be obtained by using any other source of electricity.

Recently, the electric light generated by a Gramme machine has been exhibited on the Victoria Tower of the Houses of Parliament. The machine is placed in the vaults of the House of Commons, near to the boilers, and is worked by a small engine, which was already there, and was convenient for the purpose. From the machine two copper wires, half an inch diameter, are led along the vaults to the base of the clock tower, and thence upwards to the signalling point, a total length of nearly 900 feet, being about three times the distance that an electric current has ever before been conducted for a similar purpose. The signalling apparatus is placed in a lantern 5 feet high, 4 feet wide, and having a semi-circular glazed front, which projects from the lantern of the belfry on the north side of the tower, or that overlooking the Victoria Embankment. It consists—first, of a fixed table, in which is inserted a flat brass ring 16 inches diameter and 1 inch broad, which serves as a roller path for the apparatus carrying the lamp and reflector; next, there is a circular revolving table, having bearings on the roller path, and which is moved around a central pivot projecting from the fixed table, being actuated by a worm wheel and screw. By means of this arrangement the light can be directed horizontally from side to side through an arc of 180°. It could, of course, be made to sweep the whole of the horizon, but the position of the lantern with regard to the clock tower is such as to enable the light to be seen through the range of a semi-circle only. Upon the revolving table, and hinged to it at the front is the elevating table, which has a screw adjustment to the rear by which the light can be raised or depressed, being capable of vertical training through an arc of 25°. On the elevator is placed the lamp

table, upon which again is a sliding platform, on which the lamps themselves stand. There are two lamps, which are in use alternately, the carbon points lasting but four hours, while the House frequently sits for ten.

The copper conductors terminate at the fixed part of the machine, and the method of carrying the current from them to the lamps is very ingenious, the moving parts of the apparatus forming in themselves conductors. The negative conductor is placed in metallic contact with one hinge of the elevator table through the centre pin on which the table revolves, and the positive conductor with the other hinge by means of the brass roller path. The currents from those points are conducted to the lamp table, and thence through the traversing platform to the lamps, metallic contact being obtained throughout the whole circuit by means of flat springs moving over flat surfaces. The changing of the lamps is effected, without any appreciable break of continuity in the light, by means of the traversing platform on which they stand, and which has a sliding motion from side to side. When the carbon points in one lamp are nearly consumed, the traverser is quickly shifted from right to left, or *vice versá*, as may be necessary. The break of contact is but momentary, and only exists during the time required to move the traverser rapidly through a space of six inches. The light will not become extinct during that period, as there is not sufficient time to allow the incandescence of the carbon to entirely subside. The springs under the lamp thrown out of use are by this action removed from the metal plate in the lamp table, and the springs under the fresh lamp are brought into contact, and the light is at once produced anew.

The intensifying apparatus at present in use is a holophole lent by Messrs. Chance, and through which the rays are sent in parallel lines. It is 21 inches in diameter, and is composed of lenses, surrounded by annular prisms, the centre part refracting the rays and the outer rings reflecting them. Should the electric light be adopted, a special lens will be constructed, by means of which the rays will be diffused through an arc of 180°, instead of being sent in one direction only. The cost of this electric light is at present estimated at 10d. per hour.

It may be of interest if we consider some matters of scientific interest in connection with this machine. In the first place, it possesses an enormous advantage over the voltaic battery in the absolute constancy of the current so long as the velocity of rotation is uniform. In an experiment carried

on for eight hours with one of the first machines constructed, the deviation of the needle of a galvanometer was absolutely invariable. Again, a voltaic battery is a complicated piece of apparatus; for each element consists of four separate solid pieces (the outer cell, the porous cell, the positive and the negative element) and two liquids, whilst in most experiments a considerable number of batteries is required. From this multiplicity of parts a voltaic battery is subject to many accidental derangements, which are likely to weaken if not destroy its power. With the magneto-electric machine there is no complication. All the parts are solidly connected together, and no special care is required.

It must also be remembered that a powerful voltaic battery costs almost as much when it is at rest as when in action. The magneto-electric machine, on the contrary, costs nothing when it is not producing an external current. This may be understood in two senses. It is, of course, evident that when no current is required the rotation of the machine may be stopped; but it is a remarkable fact that, even when rotation of the armature is still going on, no mechanical force is expended except that necessary to overcome friction, provided the exterior current does not flow. To understand this, let us examine a little more closely into the working of the machine. In the first place, suppose the machine to be in rapid movement, and furnishing a current in an exterior circuit, it will be observed that the armature does not get hot; from this it may be concluded that all the mechanical force transmitted to the machine is converted into electricity, since none is changed to heat. In the next place, the machine continuing to revolve with the same speed, suppose the exterior circuit to be broken; still the machine does not rise in temperature, showing that in this case there is neither production of heat nor electricity, and consequently no waste of mechanical force. From the way in which the currents in the armature are generated, when there is no exterior circuit along which they can flow, they neutralise one another, and keep in such perfect equilibrium that there is absolutely no circulation, and consequently no heating.

If the Gramme machine is set in motion by a force just sufficient to turn it with a definite velocity when the exterior current is flowing, and if the outer circuit is suddenly broken, the machine is seen to acquire an increasing velocity, showing that the mechanical force applied to it, being no longer capable of going off as electricity,

spends itself then in augmenting the velocity of the moving parts of the machine.

On the other hand, if the machine is kept at a certain speed of revolution whilst the outer circuit is broken, and the circuit is then suddenly closed, the speed instantly diminishes, showing that a portion of the force turning the machine changes into electricity.

These experiments show that, whether the machine be active or passive, there exists always a state of equilibrium between the expenditure of mechanical force and the production of electricity.

IV. THE MINERAL RICHES OF THE PHILIPPINES.

By W. W. Wood, Hong Kong.

SPAIN has always prided herself on being a "*nacion minera*"—a mining nation—and there are few countries in which so great a variety and abundance of mineral wealth is found as in the Peninsula, where many of the mines have been worked from very remote times. Notwithstanding the mining colleges and administrations, and in spite of many modern inventions for saving time and abridging labour, much of the gear used in the Spanish mines is to this day of a very primitive description, and the produce of the ores less than that obtained by the adoption of modern machinery and improvements in smelting. This being the case in the mother country, it is not difficult to imagine that in a remote colony like the Philippines, and one in which as yet little has been done towards exploring the country from a mining point of view, the devices for the extraction, &c., of ores are of a still more simple and ineffective kind. In addition to this, a very misplaced economy, or perhaps want of means, has prevented several mining adventures, undertaken in various parts of the Archipelago, from resulting favourably for the projectors.

The interior of the great islands of Mindoro and Mindanao are but little known, but from their extent and variety of surface are probably rich in minerals; but geological surveys are extremely difficult, owing to the extent and impenetrable nature of the forests which cover the greater

part of them. Whatever reports may have been made by the Spanish mining engineers (of which corps a certain number are stationed in the Philippines), with one or two exceptions, they have not been published, and are buried, with other similar documents, in the archives, and practically inaccessible.

The mining operations of the natives are conducted in the simplest and most imperfect manner, and the natural consequence is that the product is of inferior quality, and the ore not fully reduced. It is so difficult to persuade this indolent and perversely ignorant people of the advantage of adopting any modern improvement, that in the manufacture of one of the greatest products of the country—sugar—it is only within a very few years that Indians rich enough to afford proper machinery for crushing the cane and boiling the juice have been induced to adopt the iron mills and proper boilers. In what are called the mines here (with a very few exceptions) the processes are precisely the same as they were before the arrival of Europeans in the colony. The theory of the native that what was done in the time of his grandfather is good enough for him at the present day, is too firmly rooted to be abolished. In addition to this, the greater part of the mining adventurers here are of a class which, having little or no means beyond their hands and a few of the rudest implements, prefer the half-idle and half-gambling life of a gold-seeker to the more solid results of constant and well-directed labour in other directions. Great ignorance is the constant source of disappointment. The most common minerals are mistaken for valuable ores, and time after time it has been the disagreeable duty of the writer to disabuse enthusiastic speculators who in the common iron pyrites thought they had discovered a vein of gold, or in the brilliant arsenical pyrites a deposit of platina. Nor is it easy to convince these people of their mistakes, for, being a very suspicious race, and generally unscrupulous, they fancy others are trying to circumvent them, and appropriate their so-called discoveries, as many of them would do to others if opportunity offered.

Gold, being very generally disseminated through the Archipelago, has naturally, from its value, and the comparative ease with which it is reduced from the ore, been the principal object of search in the Philippines. Silver (argentiferous galena) is rare, but there are innumerable points at which gold may be found; the greater part of the metal being that of "lavaderos," or washings of rivers and small streams. In such situations it is found in minute

scales, generally in the sand. In the province of South Camarines there is a gold-bearing quartz which, even with the imperfect appliances of the Indians, has produced good results; but as their operations are confined to sinking pits which soon fill with water, which they are unable to get rid of, the works are on a very small scale. Some years since a Spanish Company—the Golden Anchor—projected a tunnel into their hill of quartz, with a view to drainage, but after expending some 20,000 dollars (a very inadequate sum) the project was abandoned, and the money wasted. The impatience of the shareholders induced them to give up the speculation I think prematurely. With the modern crushing machines it is not improbable that the Camarines gold-quartz would yield handsomely, especially if proper means were taken to drain the mines, in order that they might be worked at all seasons of the year. A curious crystallised variety of native gold is brought from Misamis and Cugayan, in Mindanao, but the gold from that quarter is said generally to be of an inferior quality to that found in Luzon. Small *pipitas* are also brought thence, though persons who discover mines are compelled, in case they wish to work them, to "denounce" them, as it is termed in Spanish mining phraseology, to the Inspector of Mines, who grants a privilege which is forfeited in case the works are not commenced within a specified period. I have never heard of any of large size being found.

One of the obstacles to progress is the enormous over-legislation in all departments of the state. Innumerable regulations restrict almost every kind of enterprise, and many of the laws are allowed to become a dead-letter for years, when they are suddenly revived, to the dismay of those who have proceeded in the supposition that their operation was really suspended. This is one of the causes why mines have languished in the Philippines. A permission to work can almost always be obtained, but the process is very frequently tedious, and to a mere Indian who has made a discovery, it is a somewhat formidable affair to obtain permission to profit by it before some unscrupulous person has taken advantage of it. The consequence is that all the gold produced in the Archipelago is the result of small washings in the rivers, or a pit sunk in the rock, which is soon filled by water, and then abandoned. The washing process is perfectly simple, the auriferous sand being scooped up, and washed in a peculiar kind of basket made of bamboo. The gold produced from ore is generally sent to market melted and cast in a shell, or any simple

mould. Much of this fused metal abounds in sulphur, which the natives are said to mix with it, and it thus becomes exceedingly brittle or *short*, breaking up when rolled. The Manila mint very properly refuses this sulphurous gold, which is sent to China, and purified there by a tedious process. The gold *dust* is generally free from adulteration, but since the introduction of galvanic gilding the greatest care is now necessary in purchasing.

Gold is common in many parts in the Philippines, and there is scarcely a stream in which it is not found in greater or less quantity. I have seen a gold-seeker at work in one of the numerous branches of the river which intersect the suburbs of Manila. The metal was washed out of the sand in minute spangles.

Gold is also collected by the various tribes of independent savages which still inhabit the centres of the larger islands, and occasionally curious ear-pendants are obtained from them, made to represent deer and other wild animals. No reliable statistics of the total production of gold in the Philippines can be had, as there are no means of ascertaining how much of the quantity found is used in the islands for jewelry and ornaments, while a good deal of that which comes to market is carried away by Chinese, who say nothing about it.

In order to supply the demand for the new gold coinage, it has been necessary, after exhausting an immense amount of Mexican and South American doubloons (the former gold currency here) to import large quantities of this metal from China, California, &c.

Gold has occasionally been found accumulated in certain parts of mountain streams in considerable quantities. An old friend of mine who was for some years governor of one of the southern provinces related to me the history of a rich find of this kind. Three Indians, professed gold-seekers, went into partnership for the purpose of exploring the upper part of a river in which they were in the habit of washing for gold. One, the uncle, furnished the supplies for the expedition, and his two nephews were his associates. They found in a deep hole where the stream fell from a height, a large quantity of gold dust, washed down by the river, and which from its gravity had settled in this spot. On their return a quarrel ensued as to how it was to be divided—the uncle claiming the largest share, as having provided the means. A lawsuit was the consequence, and meanwhile the treasure was deposited with the Governor, who assured me that the metal was of the finest touch. Subsequent

search about the same locality by others resulted in disappointment, no similar deposit being found.

In the gold-mining quartz which I have seen, the metal is either disseminated throughout the gangue in small specks, or is found in the form of thin sheets about the thickness of paper, and this is laboriously picked out from the crushed mineral with infinite labour, as the process of amalgamation is beyond the means of the native gold-miners. The gold found in the Philippines is generally bought up by Chinese or Mestizo dealers.

I am not aware of the existence of any silver mines in the Philippines. Some years ago a very rich deposit of argentiferous galena was discovered in the Island of Luzon. The ore was said also to contain a notable quantity of gold, but the affair was kept very secret. An assay having been made in Madrid, a company was formed there, and the principal shareholders were said to be Queen Christina and the Duke of Rianzures. The ore was shipped quietly to Spain, no attempt at reduction being made here, and after some time the enterprise was abandoned, as the vein became exhausted. There is no doubt that the ore was very rich, and as a regular miner was sent out to direct the work, it is probable that the mine was well worked out before it was closed.

Platina is said to have been discovered in the mountains of San Mateo, to the north-east of Manila, a great many years ago, but the story, which is the following, appears to be very doubtful. Some Indians showed to the curate of one of the villages of the district some grains of what was said to be a hard white metal, samples of which were reported to have been declared true platina by the Mexican Administration of Mines, to which they were sent. The padre naturally desired to visit the locality whence they came, but the natives always excused themselves, as they dreaded being forced to work the mine. After a great deal of threatening and persuasion, they at length consented to lead the padre to the spot, on condition that he permitted them to blindfold him. He was accordingly taken in a hammock to the place, and rapidly returned to his village. The next morning his guides disappeared, and never again made their appearance. No similar discovery has since been made.

The most important mining operation in the Philippines is that of Mancayan, where a very rich deposit of antimonial or grey copper has been discovered, and worked for some time by a company, few of the original shareholders of

which have now any interest in it, the usual impatience for grand results having caused most of them to sell out. A great deal of money has been spent in this undertaking, and it is now only beginning to yield a favourable return. The great objection to the mine is that it is situated at a considerable distance from the coast, from which it is separated by a very rugged mountain chain, intersected by deep gullies or *barrancos*, making transport both difficult and expensive. The works are conducted scientifically under experienced engineers, and the metal produced is of good quality. An immense number of obstacles have presented themselves in the course of the work, but which patience and skill have in a great measure overcome, but the great difficulty of the road to the coast is insurmountable, except at an expense which it would not be prudent to incur. The ingots of copper are carried down by natives on their backs, and the stores, &c., for the mine reach it by the same conveyance. The indomitable courage and perseverance of the original projector, and present head of the enterprise, have alone prevented its being abandoned long since. In a more favourable situation there is little doubt that the copper mine of Mancayan would be a most prosperous concern. The ore is generally massive, but some very beautiful crystallised specimens have been brought to Manila. Long before Europeans began to work there, the savages of the interior smelted this rich ore in their rude way to form pots and kettles, which appear to have been beaten out of masses of the pure metal. These are now becoming rare, and are considered great curiosities in Manila.

Great hopes were at one time entertained of larger profits from copper found in the Island of Masbate, but this, like so many other attempts made here, resulted in failure. The ore, which in many cases consisted of almost pure native copper in nodules and irregular masses, was found at a trifling depth, disseminated in the soil, but I believe never in sufficient quantity together to make it a paying speculation. This probably was partly caused by the want of mining experience, and a disinclination to risk expenses except for a certainty. A good deal of money has been wasted in the Philippines on mining speculations undertaken without experience, and carried on without the knowledge necessary to success. One of these days we shall very likely hear that great mineral riches have been discovered, and are worked successfully, for there can be but little doubt, from what is already known, that such

must exist; but if this does take place, our friends, the Spaniards, must change their present system of parsimony in their operations, or perhaps the result may be brought about by foreign energy, and the introduction of foreign capital. Mines in Spain are now worked profitably by foreign companies which yielded but poor results to the Spaniards themselves.

No lead mines have yet been worked in these islands with the exception of that of argentiferous galena already alluded to under the notices of silver, though extremely rich samples of ore have been sent to Manila for examination from both the Island of Cebu and the province of Camarines north. Not having visited the localities in which the specimens were found, I cannot say how far it would be worth while to work them. In the neighbourhood of Labo, in North Camarines, and not far from the point where galena has been discovered, is a deposit (said to be now nearly exhausted) of that rare mineral the *chromate of lead,* which hitherto has been brought almost exclusively from Siberia and Brazil. Soon after its discovery, a great number of most magnificently crystallised specimens were obtained from an excavation made by a Spaniard, who is said to have gone to the expense of having it filled up again, in order to be the sole possessor of these fine minerals. In this he was disappointed, however, as much more of the chromate was obtained, though not in such large or such perfect specimens. The crystallisation variety is accompanied by a much larger quantity of an earthy and massive kind. The natives have destroyed quantities of the crystals by pounding them to powder, to be used as sand for drying writing, and make use of the galena in the same way. I have had a large bottle filled with small crystals of chromate picked from the gangue and intended for the above purpose.

Iron appears to be abundant, and the ore very rich at what are called the mines of Augat, in the Province of Balacan. Some attempts have been made at work in the European style, but from various causes this enterprise has failed like so many others. The iron is now obtained from sample pits, dug by the natives, who reduce it in the most primitive of furnaces. The chief use to which the metal is applied is in castings for the ploughs of the country, which are very rude and ineffective implements. From Augat have been received some good specimens of magnetic iron (native loadstone), and some of the samples of ore are of the richest description.

In former days, iron was procured in the mountains of

San Ysidro, near Manilæ, and Government had an establishment there or near it, for the manufacture of cannon balls, &c., all of which have long been abandoned. It is said that many years ago a private individual (or company) projected works upon a large scale at this point, and machinery was imported from Europe. This also failed from the almost inaccessible nature of the locality, and the writer was assured by a friend that when he was a young man he had seen on the mountain road large pieces of iron castings, shafts, &c., which had been there left in despair by the owners, who found it impossible to get them to the mine over mountains and barrancos entirely destitute of anything in the shape of a road beyond a mere footpath.

The history of a deposit of metallic mercury in one of the southern provinces of the Island of Luzon is enveloped in a certain mystery. The only intelligible account which I have been able to obtain, and which reached me with a fine sample of quicksilver, is this:—At a point on the sea shore there is a high bank of clay, and below this clay a stratum of magnetic iron sand. By making cavities in this sand the pure mercury is found filtered into them, and when a pit is sunk through the clay into the sand, after a few hours a quantity of the perfectly pure metal is collected. No traces of any ore have been found, most probably from the fact that no one competent has taken the trouble to investigate the matter. It was said, I know not with what truth, that Government discouraged any further researches, fearing that the discovery and production of mercury in the Philippines might have a disadvantageous effect upon the mines of Almaden in Spain. One of the mining engineers assured me that the pure metal was found in the way described. This is certainly a very singular affair, as deposits of quicksilver, though occasionally occurring (as in the mines of Istria), are much less common than cinnabar, from which the greater part of mercury in commerce is derived. A story was reported that many years ago a vessel was wrecked on the coast, having a quantity of quicksilver on board, but this would seem a very lame explanation. I have particularly inquired whether anything resembling the soft shale in which globules of mercury are visible was found in this neighbourhood, but have not been able to ascertain more than what I have already related.

Coal is found in several localities in the Philippines, but the only mines which have been regularly worked are on the Island of Cebu—private enterprises which have cost much more than they have yielded. The great obstacle to

these undertakings, as well as to large agricultural establishments, is the difficulty almost always experienced in getting labourers. Chinese have been tried, but with very indifferent success, as the contract coolies generally become worthless, idle, and dissipated; while the natives have a strong aversion to anything like continued labour, excepting where they are proprietors. A native will work pretty hard for a week to gain enough to enable him to be idle for a fortnight. They require constant watching and urging, and never do anything thoroughly unless obliged to do so by unwearied superintendence. In addition to this there is the greatest dislike to mines and subterranean work, though well remunerated.

The quality of the coal hitherto obtained is not very good. As it contains pyrites it is very liable to spontaneous combustion, and in one of the mines the escape of inflammable gas has already caused some accidents, which have increased the prejudice of the natives against this kind of labour. The two principal mines in Cebu are situated in a range of mountains, which runs longitudinally through the middle of the island, and are about five miles from the coast. One of the principal expenses has been the formation and maintenance of roads, which suffer a good deal during the rainy season.

The Cebu coals have been found excellent in Hong Kong for the production of gas, though of inferior quality for steamer use.

Coal is by no means confined to the localities of the two above-mentioned mines, but is seen cropping out at various points of the mountain range. The distance to the coast is an inconvenience, and from the nature of the country, transport can only be made in carts drawn by the buffalo, a very slow mode of conveyance. Work has ceased in one of the Cebu coal mines, and it has been offered for sale: and the other is worked imperfectly, and on a small scale.

Perhaps as the shafts are deepened veins of better coal may be found, but a great deal of capital and machinery would be necessary, and all things considered it would be a very bold enterprise to attempt; though from the increase of steam vessels in the Archipelago within a few years past, there is no doubt that large supplies of good coal would find ready sale, that used here at present coming either from England or Australia, and costing proportionately high. Spanish capital is not likely to be employed here in mines; and until greater security is offered by Government, and

labour is more readily and constantly to be had, there is little hope that foreign speculators will be tempted to adventure such large sums as are indispensable to the successful working of mines.*

Lignite occurs also in several places, and a good deal of disappointment has been suffered by persons who fancied that in the deposits of this mineral in favourable situations they had fallen upon a true coal.

The only available building stone in Manila and its vicinity is a species of volcanic tufa, which composes large quarries, from which the whole city has been built. In breaking large masses of this stone, which when fresh quarried is very soft, fossil wood is frequently found imbedded in it, as well as branches of trees which have not become silicified, and occasionally fragments of *charcoal*.

In Cebu and some of the southern islands the coral reefs are resorted to for building materials as well as for lime. Limestone is found only in a few localities, but is much used for the latter purpose.

Some very pretty marbles are quarried in the island of Romblon, and at various other points handsome varieties are found which are now worked in Manila for church fonts, "sepulchral tablets," and ornamental purposes.

Good samples of gypsum (which are accompanied by the anhydrous variety) are found in the Province of Butangas, and large quantities of a fine kaolin are produced by washing the *débris* of certain decomposed rocks which are common in many places. The only use to which this valuable material is applied is for the preparation of a fine whitewash for interiors. A cold spring near Bäy affords splendid specimens of a siliceous deposit, which invests twigs, leaves, &c., which fall into the water. At Tibi, in Albay, the thermal springs also deposit silex, and some of the specimens brought thence are very remarkable. The water of these springs is hot enough to cook an egg in a few minutes. On the lake of Bäy are others which, however, do not leave any notable deposit.

In the foregoing account of the mineral productions of the Philippine group, I have avoided as much as possible any attempt at geological description of the localities, from the fact that I have visited some of them only and in too

* The jealous feeling which so long excluded foreign enterprise from Spain and her colonies, and the indifference which prevented the Spaniards themselves from profiting by the riches within their grasp, is beginning to give way at last. How disgusting it must be to them to recollect that when masters of California they thought it good for nothing but a pasture for cattle.

hurried a manner to enable me to furnish reliable particulars. As I have already remarked, geological examinations are difficult, from the deep forests which cover the interior. At the Cebu coal mines, which are situated in a valley between two ranges of hills, into the second of which the shafts are driven, I observed in the cuttings made along the shoulders of the hills several old sea-beachers, one above the other, at an elevation of some 500 feet above the level of the sea, composed chiefly of rotted masses of coral, some of them very large; and close to the coal the indurated clay filled with casts of fossil shells, most of them bivalve and very difficult to determine.

The matrix of the Mancayan copper is, I believe, a porphyritic rock, but cannot speak with certainty.

At one point in Cebu is noticed some masses of grey marble with white veins, blasted from the side of the hill in making the road to the mine, so handsome that, were the distance from the coast less, it would be profitable to quarry it for sale at Manila.

No general geological survey has ever been made by the Spaniards, and nearly, if not all the discoveries of minerals in the islands, have been made by the natives in their search for the precious metals.

Considerable quantities of impure sulphur are brought to Manila from the island of Samar, where it is dug and fused in large blocks. It is said that much more might be had were the demand for it larger.

The bottom of the crater of the volcano of Zual, in the province of Batangas, about 40 miles from the capital, would also furnish a good deal of sulphur, which is condensed from two or three *fumaroles,* almost constantly ejecting sulphurous vapour, in which the north-east monsoon condenses upon the leeward side of the crater, giving the ledges of the rocks the appearance of being covered with snow.

I am not aware that antimony, which is so abundant in the neighbouring island of Borneo, has ever been discovered in the Philippines, nor have any deposits of tin been found. Zinc, as *blende,* accompanies the galena, and in the north, near Mancayan, there are quantities of iron pyrites, which are brought to the mines by the semi-wild natives of the district to be used in the smelting operations. In Mindanao, near Surigao, I have found very fine arsenical pyrites most beautifully crystallised.

There can be little doubt that much has yet to be discovered in the way of valuable minerals in these islands,

but under the present system there is little encouragement to invest money in mining operations.

With regard to mineral springs but little is known. Streams strongly impregnated with iron derived from decomposing rocks are common, and a strong sulphur spring exists near Irlajala.

V. NOTES ON RECENT CHANGES IN BRITISH ARTILLERY MATĒRIEL.

By Captain S. P. Oliver, Royal Artillery, F.R.G.S.

THE following notes embrace the more important results of the investigations and experiments carried on by various permanent and special committees in connection with the department of the Director-General of Ordnance in continuation of those last noticed in this journal in No. XXXIV., April, 1872.

The reader will observe the steady progress in our knowledge of explosives, an improvement in their manufacture and in the construction of our heavy ordnance and its adjuncts for obtaining increased accuracy, together with various nice mechanical contrivances for rapidity of traversing, elevating, and general working of the modern ponderous weapons.

I. The investigations of the special committee on guncotton and lithofracteur first claim attention, on the score of the vast importance of the results obtained, which cannot fail to have a great effect on our future war *matériel*.

This committee was first appointed in September, 1871, for the purpose of considering and reporting upon the general question of the manufacture, storage, and use of gun-cotton and lithofracteur, and was composed as follows :—

President—Colonel C. W. Younghusband, R.A.

Members—Colonel T. L. J. Gallwey, R.E. ; Colonel T. W. Milward, C.B., R.A. ; Lieut.-Col. C. B. Nugent, R.E. ; Captain E. Field, R.N. ; G. P. Bidder, Esq., C.E. ; Dr. W. Odling, F.R.S. ; and H. Bauerman, Esq.

Secretary—Captain W. H. Noble, R.A.

The following are the leading points which the Committee are required to investigate :—

1. Whether the employment of gun-cotton is attended with such uncertainty or peril as should induce the department to relinquish its manufacture, and its use for those military purposes for which it has hitherto been considered peculiarly valuable.
2. Whether its manufacture, in all its various stages, is a dangerous process, and one that should not be carried on near an inhabited neighbourhood, and whether additional precautions to those now in force seem necessary.
3. Whether the storage of gun-cotton, either wet or dry, is necessarily attended with danger in magazines, on shore, or on board of ship, under any or all conditions of temperature.
4. Whether, either in a pure or impure state, it is liable to spontaneous combustion, and, if so, whether such combustion would result in explosion or in mere ignition.
5. The nature of buildings best suited for the storage of gun-cotton.

The committee will, however, report upon any points in addition to those above enumerated which may arise in the course of their investigation, and to which they may consider it desirable to draw attention.

The question of safety, for transport and storage, of the substance called "lithofracteur," is also to be investigated by the committee.

The committee, after a careful review of the documents in their possession, and of the evidence of officers and others respecting the use and application of compressed gun-cotton, principally as regards its employment for military purposes, consider that its use is not only unattended by either uncertainty or peril, but that the material as an explosive agent is effective, certain, safe, portable, and easy in employment. They therefore feel that they are warranted in the expression of a strong opinion of its great value for military engineering purposes generally, and for submarine mining.

As regards *storage* no extended experience has been gained by the officers who have used it at Chatham and elsewhere, but within the limits of twelve months no change has been observed.

The evidence respecting the *stability* of a material which has been in practical use during a comparatively short period, is necessarily meagre, time forming an essential element in determining upon this important quality.

The committee find that considerable quantities have been sent during the past two or three years to hot and damp climates, and have undergone voyages to Australia and India without, so far as they can learn, any accident whatever. They have also learnt that some gun-cotton which was supplied by the Stowmarket Company in the summer of 1870, and kept in a magazine on the Thames, was subsequently sent to Calcutta, where it has been stored for some months. A report recently received from Colonel Kennard states that the gun-cotton shows no indication of any change.

The reports published in Austria furnish very satisfactory evidence respecting the stability of gun-cotton, and a consideration of them, together with the other evidence adduced, has satisfied the committee that no hesitation need be felt in continuing the employment of compressed gun-cotton through any fear of undiscovered unstable qualities.

They have examined a considerable number of specimens of gun-cotton, some of them purposely left impure, that have been stored at Woolwich for several years past (several specimens for periods as long as nine years) under varying conditions of exposure to light, heat, and change of temperature. Their present unaltered state furnishes fully confirmatory testimony that under at least all ordinary circumstances gun-cotton may be regarded as a stable material.

The experiments on stability of gun-cotton, extending over a long period, refer to the material in the form of *rope* or *skeins*, that is gun-cotton in the loose state, as distinguished from the substance compressed into blocks, or discs from pulp, on Mr. Abel's system. But as it has been satisfactorily proved to the committee that gun-cotton produced from the long staple cotton cannot be so perfectly purified as pulped gun-cotton, it follows that all the evidence in favour of the stability of gun-cotton in the loose state applies with much greater force to compressed gun-cotton, in the purification of which the pulping process has been applied.

As regards *manufacture*, the committee have made themselves acquainted with the nature of the several processes constituting Mr. Abel's system up to the stage in which gun-cotton is compressed into discs and ready for use.

In all these processes the material, from the moment of its conversion into gun-cotton and up to the drying stage, is in a wet state, and at the final stage of leaving the press contains from 15 to 20 per cent of water. It is throughout

every stage perfectly uninflammable, and the committee are therefore satisfied that no danger can possibly result from its manufacture (with the exception of drying) in any locality, whether in or near a town.

The operation of drying, as followed at Stowmarket, seems to be open to some objections, but the committee apprehend that no difficulty will be experienced in a safe and simple method being devised, which may be easily applicable to any locality, and feel no hesitation in recording their opinion that there is no reason why the War Department should relinquish the manufacture of compressed gun-cotton.

Mr. Abel, chemist to the War Department, describes the results of some experiments he has made with certain modifications of gun-cotton by incorporating the pulp with an oxidising agent. The results appear to point to great improvements that may be effected in manufacture, attended with considerable increase of efficiency, and may be summarised as follows:—

1. There is no difficulty in incorporating with gun-cotton pulp, nitrate of potash, nitrate of soda, or chlorate of potash, in such proportions as will get the full amount of work out of the carbon in the cotton-wool; and in afterwards pressing the mixed substances into masses, as easily as ordinary compressed gun-cotton.

2. Discs made with any of these mixtures are hard and compact, and less liable to split than discs of pure gun-cotton.

3. They can be coated with a waterproofing composition, and can then be used for blasting in wet holes.

4. They are less ignitable by flame than pure gun-cotton discs, and when ignited burn more slowly.

5. When exploded by detonation, the products of combustion furnish little or no carbonic oxide, the presence of which renders the use of ordinary compressed gun-cotton objectionable in military mines.

6. Gun-cotton, mixed as described, may be kept wet like ordinary gun-cotton; and in the dry state the mixed material seems to resist the action of continued exposure to high temperature for a far longer period. It would, therefore, be more stable, and the objections to storing dry gun-cotton in any climate might be removed.

7. The cost, weight for weight, of the mixed gun-cotton is much less than the ordinary material; and the production can be increased without any additional plant.

Special Committee, 8/4/72, consider that the above

results are so important, and their realisation will acquire so high a value as bearing upon the employment of gun-cotton for military purposes, that they think the subject should be at once fully taken up.

Recommend that Mr. Abel be authorised to proceed with his experiments, and to prepare for trial samples of the mixed substances as above described.

The principal points which the committee think deserving of investigation are:—

1. Whether, when saltpetre is incorporated with gun-cotton pulp, the mixture can be compressed into discs which possess advantages over discs of pure gun-cotton in being *harder, less liable to flake, less inflammable, more stable, and not less efficient.*
2. The exact proportion of saltpetre which must be incorporated with gun-cotton pulp to develope the same amount of explosive force as pure gun-cotton, weight for weight.
3. An investigation into this and other properties of gun-cotton incorporated with nitrate of soda and chlorate of potash.
4. The cost of each of these modifications of gun-cotton, compared with ordinary gun-cotton; and their relative rate of production.
5. The special military uses for which each of them seems particularly suitable.

An important improvement in the manufacture of gun-cotton has been suggested by Mr. Abel, and the expectations he had formed have since been fully confirmed. The improvement consists in the addition of a proportion of ammonia to the gun-cotton at, or soon after, the commencement of the washing process; the effect of which is, not only to neutralise any free acid that may be left in the beaten-up pulp, but to act as a powerful solvent in removing the resinous and other organic matters locked up in the fibres of the cotton, the presence of which materially interferes with the stability of the finished product.

The importance of this step in manufacture may be judged of by the fact that, whereas the poaching or washing operation has hitherto required the use of *warm* water, and has had to be continued for a period of *sixty hours as a minimum*, but extending sometimes to *ten or even fourteen days* before complete removal of these substances and consequent purification is obtained, the ammoniacal washing completes the operation with cold water in about *twenty-four hours.*

This new development effected in the manufacture of

pulped and compressed gun-cotton will result in a considerable saving of time and labour and in the employment of a less extensive plant.

On the 25th April, 1872, two Martello towers between Hastings and Winchelsea were destroyed by the Royal Engineer committee with gunpowder and gun-cotton in order to compare their effect in hasty demolitions. Both demolitions were considered perfectly successful, and showed that the proportion of four to one in the weights of gunpowder and gun-cotton to produce the same effect was practically correct. On the results of this experiment the committee arrive at the following conclusions:—

1. As gun-cotton is not materially, if at all, injured by being kept in a damp state, and as the operation of drying can be easily carried out, it is unnecessary to store it in the dry state, and the committee think it should not be stored dry in larger quantities than are required for the current wants of the service. Apparatus for drying should be established at all stations where dry gun-cotton is required for use.

2. The present service pattern-box is objectionable for packing dry gun-cotton; its strength is an element of danger, in the event of the accidental ignition of a store of gun-cotton packed in such boxes; and it is unnecessarily strong for transport.

3. In a store of any construction, the ignition of large quantities of dry gun-cotton packed in strong boxes will be followed by violent explosion; but in lightly-made boxes, or in boxes designed specially to facilitate the escape of the heated gas before it has reached the exploding point, and in magazines lightly constructed, ignition will probably not be followed by an explosion; but the Committee are of opinion that the experiments recorded do not afford a sufficient guarantee that ignition will not be followed by explosion if the quantity, however stored, be very large, or the building be exceptionally strong.

4. Taking these points into consideration, the committee think that dry gun-cotton, wherever stored, and in whatever quantity, should be treated as an explosive, and that the precautions now observed with explosives generally, as regards locality and description of building, should also apply to gun-cotton.

5. Gun-cotton in the wet state being perfectly uninflammable, no special regulations are necessary for its transport; in the case of dry gun-cotton, which under ordinary conditions is non-explosive, but readily inflammable, the

committee are of opinion that it may be safely moved under the regulations which may govern the transport of gunpowder.

6. The evidence obtained by the committee tends to show that pure gun-cotton is a stable material, but experience on this point is limited. They think it, therefore, preferable at present to follow the more prudent course of excluding it from magazines containing gunpowder; although they consider that gun-cotton may be stored, when convenient to do so, in magazines built for gunpowder.

It should, however, be understood that when circumstances absolutely require it, such as when a second safe store is not available, dry gun-cotton may be temporarily placed in a magazine with gunpowder.

7. The recommendations of the committee in their preliminary report, with respect to wet gun-cotton, require no amendment.

Since the above conclusions were arrived at by the special committee, the discovery has been made that compressed gun-cotton can be exploded *when wet* by means of detonation; this discovery has been fully confirmed by the results of some experiments lately carried out at Weston-super-Mare: and it is not improbable that moist gun-cotton will be utilised for bursting charges of shells in future.

On the 4th April some further trials were made near Eastbourne to determine the liability or otherwise of stores of wet gun-cotton to explosion from simple inflammation. Accordingly, two magazines were prepared at Pevensey, in each of which one ton of Abel's compressed gun-cotton discs was placed, containing 30 per cent of moisture. In one the gun-cotton discs were packed in 80 regulation boxes with their lids screwed down, and in the second the discs of gun-cotton were removed from their boxes and placed naked in a large wooden tank.

On the application of fire to the magazines, smoke and flame issued in considerable volumes and in successive bursts as the boxes caught fire, and after two hours and a half of intense conflagration the fire died away without any explosion, the whole of the gun-cotton having been totally consumed, and the interiors of the magazines glowed like furnaces. The result of this crucial experiment was most satisfactory.

The enormous importance of these experiments, as establishing the immunity from explosion of moist gun-cotton in compressed discs in certain quantities cannot be overrated,

and the further report of the committee, which has not yet been published, will be looked forward to with interest.

It is to be hoped that the Government will not overlook the valuable properties of dynamite for mining purposes, not only on account of the additional work done by this explosive in comparison with gun-cotton, but because the products of its combustion are less injurious to the health of the miners who use it.

On the other hand, from the following abstract of the report on the lithofracteur, this substance is found too defective to be introduced into the service at *present*.

"The committee are of opinion that the substance provided for their experiments, under the name of lithofracteur, has imperfectly fulfilled the absolutely necessary property of retaining its proportion of nitro-glycerine, under circumstances which might be met with during ordinary transport or storage.

"Nitro-glycerine readily exuded from a proportion of the cartridges of the lithofracteur subjected to trial, after a comparatively short exposure to a temperature not exceeding 100° F.; and although such a temperature may not in an English climate be sustained for any length of time, either during a railway journey or in a magazine, it must be borne in mind that the nitro-glycerine once exuded may not be re-absorbed, but that fresh exudation would probably take place on each fresh application of heat, and that this tendency to leakage might be facilitated by the shaking inseparable from railway transit.

"The capacity of lithofracteur for retaining nitro-glycerine is very seriously interfered with by its becoming wetted. The nitro-glycerine is readily expelled from a lithofracteur cartridge immersed in water. The readiness with which the lithofracteur parts with its nitro-glycerine, under the influence of water, is dependent probably on the presence of nitrate of soda as one of its constituents, a substance exceedingly soluble in water; and in the event of a box of cartridges getting wet, water would replace part of the nitro-glycerine, which would thus collect as a liquid form at the bottom of the box.

"The committee regret they cannot make a more favourable report upon a substance which may possess many valuable properties for industrial purposes, but they regard the tendency of some of the lithofracteur submitted to them to part with its nitro-glycerine, under conditions that can only be regarded as ordinary, as a defect too serious to be

ignored. They hope the manufacturers may succeed in overcoming the difficulties thus indicated, and enable this explosive, so useful for many purposes, to be admitted without restriction.

"The committee have no hesitation in recording their opinion that a safe and unobjectionable nitro-glycerine compound, possessing valuable explosive properties for many useful purposes, and fully meeting all the requirements of quarry owners, can be manufactured, transported, and stored in this country."

With regard to powder, it has been found that the very heavy charges of pebble powder, although they may not give the same pressures as the former charges of rifle large grain, still really do much more real damage to the guns, and will render the necessity of *re-venting* and indeed of *re-tubing* the guns much more frequent.

II. The last experiment with regard to the constant variations in the strength of gunpowder has led to a reconsideration of the lately-existing proof regulations (one and a quarter times highest service charge) for heavy guns. It is found necessary, in consequence of the unsatisfactory discrepancies in amount of pressure produced by a comparatively small increase in the charge, that proof charges should be larger than service ones, in order to ascertain whether the gun possesses superabundant strength to resist the effects of powder which may be more than ordinarily violent; and therefore the proof of guns should be conducted upon the same general principles as heretofore, viz., by using the service projectile and a charge somewhat in excess of the service one, but the amount of excess need not be so great as of old, that the gun may not be subjected to severe local pressures or strains which might permanently weaken or otherwise injure the structure of the gun under proof. The proof charge of the 12-inch gun of 35 tons will, therefore, now be as follows:—First round 110 lbs., and second round 115 lbs. of pebble powder; the projectile of service weight being used in each case. Experiments are now being carried out to determine the proportionate increase of proof over service charge for 8, 9, 10, 11, and 12-inch (25 tons) guns. It may here be noticed that the learned Professor Bashforth is disposed to question the very discordant results obtained by the committee as to the measurements of the variation of powder-pressure, as registered by the crusher-gauges and the chronoscopes of Schultze and Noble, on whose results little reliance is to be placed in consequence of their records being received on the surfaces

of cylinders, which are subject to more or less vibration from *toothed* wheels being used to drive them. He suggests that the law of propelling pressure could best be ascertained by means of the proposed breech-loading gun of Captain Morgan, R.A., described and figured in No. XXXII. of the "Quarterly Journal of Science," October, 1871.

III. In consequence of the accidental bursting of five cast-iron smooth-bore 68-pounders and one 24-pounder gun at Madras, the special committee have made experiments with the view of testing the action and ascertaining the general nature of Indian-made gunpowders; and the results prove these powders to possess exceptionally violent properties, due principally to the highly inflammable and readily oxidisable character of the charcoal used in manufacture, which in future is to be modified. The following analysis of the charcoals, both Indian and English, will show that the Indian charcoal is not dissimilar to that used in the Spanish Government powder, which has long been known as brutal in its action and differs greatly in composition from that used at Waltham Abbey and by the English makers. The analysis of the charcoal, as well as that separated from the powders, furnished the following results, the figures quoted being the mean of two determinations:—

	Indian Charcoal.	Charcoal from Madras powder.	Charcoal from Ishapore powder.
Carbon . . .	73·92	78·70	76·63
Hydrogen . .	3·52	3·06	3·28
Oxygen, &c. .	20·22	13·56	17·68
Ash	2·34	4·68	2·41

The following are the results of the analysis of average samples of English dogwood, willow, and alder charcoal as used at Waltham Abbey, and of Spanish charcoal:—

	Dogwood.	Willow.	Alder.	Spanish.
Carbon	83·80	84·41	87·00	76·29
Hydrogen . . .	3·28	3·24	2·98	3·31
Oxygen, &c. . .	11·21	10·71	8·78	14·87
Ash	1·71	1·64	1·24	5·53

The dogwood charcoal is decidedly more inflammable than that obtained from alder, and even more so than willow charcoal, as measured by the proportions of hydrogen and oxygen contained in each; yet, though it is the most readily oxidisable charcoal made in England, being exclusively used in the manufacture of small arm powder, it cannot be compared in this respect with any of the samples from India.

IV. Colonel Erskine's committee has finished its labours, and it is satisfactory to find that they have observed throughout their investigations a marked superiority in wheels constructed in the Royal Carriage Department in regard to quality of material and workmanship over those made elsewhere.

The committee prosecuted their inquiry under two distinct heads, viz. :—

1. Wheels.

Experiments were made with eleven different patterns of wheels, consisting of—

A. The service wood, with Madras nave.
B. Messrs. Perkins and Son.
C. Messrs. Sterne and Co.
D. The Phantom Wheel Company.
E. Messrs. M'Neill and Brothers.
F. Sir W. G. Armstrong and Co.
G. Messrs. Brown, Marshall, and Co.
H. Superintendent of Machinery, Royal Arsenal.
I. Colonel Clerk, R.A.
K. Royal Carriage Department composite wheel.
N. Messrs. Holmes and Brothers.

The only wheels which passed through the trial of travelling the prescribed distance of 1500 miles over macadamised roads, rough country, and paved roads, were those marked A., B., F., K.

After the close of the experiments the committee proceeded to ascertain the relative degrees in which these four patterns possessed the properties, enumerated below, of a thoroughly efficient wheel for transport service, viz. :—

1. Strength and endurance to withstand the strains and wear to which it is liable on service.
2. Non-liability to rapid deterioration through climatic exposure in the field.
3. Capability of being easily and quickly repaired in the field.
4. Lightness.
5. Power to resist the injurious effects of long storage.

It was found that the four patterns stood in order of merit as follows :—K., F., B., A.

The distinctive features as regards material of the wheel thus proved to be the best, being iron for the felloes, tire, and nave, and oak for the spokes.

The committee give without hesitation their opinion that iron may be advantageously used for making naves, felloes,

Statement of Wheels Subjected to Experiments, Shewing Results, &c.

Name of Maker, &c.	Letter of wagon and cart.	Four wheels supplied at a cost of	Description of Wheel. Weight.			Nave.	Pipe Box.	Spokes.	Felloe.	Tire.	Miles travelled. Aldershot. Over macadamised roads.	Aldershot. Over unmade roads and rough country.	Wool-wich. Over granite pavement.	Total.	Date of ceasing to travel.	Remarks.
		£	cwt.	qrs.	lbs.											
Service wheel . . .	A	—	2	0	3	Wrought-iron flanges.	Gun-metal.	Oak.	Ash.	Iron ring.	310	600	600	1510	—	Completed travelling test.
Perkins and Son . .	B	80	2	1	9	Cast Steel.	Do.	Iron round*	Oak.	Trough iron.	310	600	600	1510	—	Do.
Sterne and Co. . .	C	75	2	0	24	Wrought-iron flanges.	Do.	Steel plate.	Beech.	Do.	310	20	—	330	15 3 71	4 wheels did 190 miles . . . (Two only the total distance given.)
Phantom Wheel Co.	D	40	1	3	19	Cast-iron.	Do.	Steel round	—	H section iron.	301	180	—	481	24 3 71	4 wheels did 70 miles . . .
M'Neill, Brothers .	E	40	2	2	6	Wrought-iron flanges.	Do.	12 sections of T iron with ash packing pieces.		Iron ring.	310	240	—	550	30 3 71	4 wheels did 490 miles . . .
Sir W. G. Armstrong and Co.	F	56	2	3	19	Do.	Do.	Iron round*	Trough iron.	Two semi-circles.	310	600	600	1510	—	Completed travelling test.
Brown, Marshall, and Co.	G	80	3	0	2	Gun-metal.	Do.	Tubular iron.	Trough iron.	Iron ring.	310	70	—	380	18 3 71	4 wheels did 330 miles . . . (Two only the total distance given.)
Superintendent of Machinery.	H	80	2	1	20	Do.	Do.	Iron, flat, curved.	Trough iron, with ash packing pieces.		310	340	—	650	5 4 71	4 wheels did 570 miles . . .
Colonel Clerk's elastic steel.	I	45	2	1	18	Wrought-iron flanges.	Do.	Steel, flat.	T iron ring with ash packing pieces.	Iron ring.	310	280	—	590	31 3 71	4 wheels did 490 miles . . .
Royal Carriage Department Madras wheel, iron felloes.	K	—	2	0	13	Do.	Do.	Oak.*	Tubular iron D section.	Do.	310	600	600	1510	—	Completed travelling test.
Holmes, Brothers .	N	67	2	3	4	Do.	Do.	Iron round.	T iron in four pieces, with ash packing pieces.		—	600	600	1200	15 1 72	4 wheels did 944 miles. Did not compete over macadamised roads.

* Weight of a spoke of B=8½ lbs.; of F=7½ lbs.; of K=4½ lbs.

NOTE.—All wheels 5 feet in diameter, tires 2½ inches, except F wheels which were 3 inches.

TABLE OF COMPARATIVE RANGES OF BRITISH AND FOREIGN FIELD GUNS.

Nation.	Nature of gun.	Calibre.	Charge.		Nominal weight of common shell filled.		Initial velocity.	Ranges, in yards.											
		inches.	lbs.	ozs.	lbs.	ozs.	feet.	1°	2°	3°	4°	5°	6°	7°	8°	9°	10°	11°	12°
BRITISH	16-pr. muzzle-loader of 12 cwt.	3·600	3	0·0	16	0·0	1355	725	1150	1560	1920	2245	2560	2840	3120	3400	3660	3920	4150
	12-pr. breech-loader of 8 cwt.	3·000	1	8·0	12	0·0	1170	580	1000	1395	1730	2045	2340	2600	2840	3065	3280	3480	3680
	9-pr. muzzle-loader of 8 cwt.	3·000	1	12·0	9	0·0	1380	660	1125	1490	1810	2095	2350	2605	2830	3040	3235	3420	3606
PRUSSIAN	6-pr. breech-loader of 8·3 cwt.	3·604	1	4·8	15	3·0	1087	410	779	1107	1410	1673	1935	2197	2446	2678	2896	3104	3304
	4-pr. breech-loader of 5·5 cwt.	3·089	1	1·6	9	6·0	1210	492	940	1290	1634	1927	2198	2460	2690	2924	3144	3362	3567
FRENCH	12-pr. muzzle-loader of 12 cwt.	4·776	2	3·0	25	6·4	1006	443	820	1115	1403	1640	1845	2047	2237	2405	2570	2720	2860
	4-pr. muzzle-loader of 6·5 cwt.	3·406	1	3·0	9	0·0	1066	415	765	1047	1305	1531	1725	1910	2080	2245	2406	2545	2685
BELGIAN	6-pr. breech-loader of 8·5 cwt.	3·622	1	5·6	15	0·0	—	475	875	1246	1585	1914	2215	2488	2756	2996	3215	3434	3653
	4-pr. breech-loader of 5·7 cwt.	3·071	1	2·7	9	3·2	1220	602	1022	1381	1723	2024	2326	2570	2822	3040	3259	3478	3675
AUSTRIAN	8-pr. muzzle-loader of 9·8 cwt.	3·830	2	0·75	14	8·0	1105	510	830	1130	1410	1675	1920	2150	2365	2565	2750	2920	3075
	4-pr. muzzle-loader of 5·2 cwt.	3·080	1	2·5	8	0·0	1054	535	835	1120	1365	1585	1795	1990	2180	2355	2520	2670	2815
RUSSIAN	9-pr. breech-loader of 12·3 cwt.	4·200	2	11·3	24	6·0	1050	470	835	1185	1505	1810	2095	2370	2630	2880	3120	3345	3560
	4-pr. breech-loader of 6·3 cwt.	3·420	1	5·6	12	10·0	1004	410	755	1075	1370	1645	1930	2170	2410	2635	2845	3045	3230
AMERICAN	20-pr. muzzle-loader, Parrott, of 15·7 cwt.	3·670	2	0·0	19	8·0	—	420	840	1260	1680	2100	2350	2600	2850	3100	3350	3565	3780
	10-pr. muzzle-loader, Parrott, of 8 cwt.	2·900	1	0·0	10	0·0	—	400	800	1180	1550	2000	2250	2600	2800	3000	3200	3400	3600
	3-in. muzzle-loader, ordnance of 7·3 cwt.	3·000	1	0·0	11	0·0	—	645	1010	1310	1525	1835	2100	2325	2400	2790	2910	3110	3270

and tires, but cannot be substituted for oak as a material for spokes.

2. Carriage Frames and Bodies.

In this instance the committee are led to the opinion that it would not be expedient to make any changes in the materials which have heretofore been used in the construction of carriage frames and bodies beyond the following ;—

1. Oak to be used universally for frames instead of ash, on account of its greater durability in store.
2. Iron angle plates to be used at the joints of all frames extending each way to a distance of from seven to twelve inches.
3. One side of all futchells and the back of splinter-bars to be faced with iron plates.

The wheel marked K, with certain modifications, is strongly recommended for adoption into the service for use with transport carriages; but the Director of Artillery does not consider that the necessity for a new pattern wheel has been established, and consequently the service pattern remains unaltered.

The table on page 340 shows the wheels experimented upon.

V. The table on page 341, showing the velocities and ranges of the English as compared to some of the continental field guns, is satisfactory. In these days of improved small arms, it is essential that our field artillery should possess long ranging powerful weapons, and in this respect it appears we are in advance of the continental armies at present.

It has been asserted that our gunnery declined during the last few years of the old French war because the enemy seldom came out of port to face it, and alarmists have not been found wanting who see in our long-continued inactivity of peace our gunnery in danger of deterioration. It needs only a visit to Shoeburyness to convince the most sceptical that there is no fear of our artillery failing through desuetude; but there is one danger which ought to kept in view, and that is, our stock of powder is limited. When we have to buy Belgian powder in time of peace, where are we to obtain a sufficient quantity in actual warfare? We need a second establishment as large as Waltham Abbey in the northern or midland counties.

VI. THE LIMITS OF OUR COAL SUPPLY.

ESTIMATING the actual consumption of coal for home use in Great Britain at 110 millions of tons per annum, a rise of eight shillings per ton to consumers is equivalent to a tax of 44 millions per annum. These are the figures taken by Sir William Armstrong in his address at Newcastle last February. As the recent abnormal rise in the value of coal has amounted to more than this, consumers have been paying at some periods above a million per week as premium on fuel, even after making fair deduction for the rise of price necessarily due to the diminishing value of gold.

Are we, the consumers of coal, to write off all this as a dead loss, or have we gained any immediate or prospective advantage that may be deducted from the bad side of the account? I suspect that we shall gain sufficient to ultimately balance the loss, and, even after that, to leave something on the profit side.

The abundance of our fuel has engendered a shameful wastefulness that is curiously blind and inconsistent. As a typical example of this inconsistency, I may mention a characteristic incident. A party of young people were sitting at supper in the house of a colliery manager. Among them was the vicar of the parish, a very jovial and genial man, but most earnest withal in his vocation. Jokes and banterings were freely flung across the table, and no one enjoyed the fun more heartily than the vicar; but presently one unwary youth threw a fragment of bread-crust at his opposite neighbour, and thus provoked retaliation. The countenance of the vicar suddenly changed, and in stern clerical tones he rebuked the wickedness of thus wasting the bounties of the Almighty. A general silence followed, and a general sense of guilt prevailed among the revellers. At the same time, and in the same room, a blazing fire, in an ill-constructed open fire-place, was glaring reproachfully at all the guests, but no one heeded the immeasurably greater and utterly irreparable waste that was there proceeding. To every unit of heat that was fully utilised in warming the room, there were eight or nine passing up the chimney to waste their energies upon the senseless clouds and boundless outer atmosphere. A large proportion of the vicar's parishioners are colliers, in whose cottages huge fires blaze most wastefully all day, and are left to burn all night to save the trouble of re-lighting. The vicar

diligently visits these cottages, and freely admonishes where he deems it necessary; yet he sees in this general waste of coal no corresponding sinfulness to that of wasting bread. Why is he so blind in one direction, while his moral vision is so microscopic in the other? Why are nearly all Englishmen and Englishwomen as inconsistent as the vicar in this respect?

There are doubtless several combining reasons for this, but I suspect that the principal one is the profound impression that we have inherited from the experience and traditions of the horrors of bread-famine. A score of proverbs express the important practical truth that we rarely appreciate any of our customary blessings until we have tasted the misery of losing them. Englishmen have tasted the consequences of approximate exhaustion of the national grain store, but have never been near to the exhaustion of the national supply of coal.

I therefore maintain most seriously that we need a severe coal famine, and if all the colliers of the United Kingdom were to combine for a simultaneous winter strike of about three or six months' duration, they might justly be regarded as unconscious patriotic martyrs, like soldiers slain upon a battle-field. The evils of such a thorough famine would be very sharp, and proportionally beneficent, but only temporary; there would not be time enough for manufacturing rivals to sink pits, and at once erect competing iron-works; but the whole world would partake of our calamity, and the attention of all mankind would be aroused to the sinfulness of wasting coal. Six months of compulsory wood and peat fuel, with total stoppage of iron supplies, would convince the people of these islands that waste of coal is even more sinful than waste of bread,—would lead us to reflect on the fact that our stock of coal is a definite and limited quantity that was placed in its present store-house long before human beings came upon the earth; that every ton of coal that is wasted is lost for ever, and cannot be replaced by any human effort, while bread is a product of human industry, and *its* waste may be replaced by additional human labour; that the sin of bread-wasting does admit of agricultural atonement, while there is no form of practical repentance that can positively and directly replace a hundredweight of wasted coal.

Nothing short of the practical and impressive lesson of bitter want is likely to drive from our households that wretched fetish of British adoration, the open "Englishman's fireside." Reason seems powerless against the

superstition of this form of fire-worship. Tell one of the idolators that his household god is wasteful and extravagant, that five-sixths of the heat from his coal goes up the chimney, and he replies, "I don't care if it does; I can afford to pay for it. I like to *see* the fire, and have the right to waste what is my own." Tell him that healthful ventilation is impossible while the lower part of a room opens widely into a heated shaft, that forces currents of cold air through door and window leakages, which unite to form a perpetual chilblain stratum on the floor, and leaves all above the mantel-piece comparatively stagnant. Tell him that no such things as "draughts" should exist in a properly warmed and ventilated house, and that even with a thermometer at zero outside, every part of a well ordered apartment should be equally habitable, instead of merely a semi-circle about the hearth of the fire-worshipper; and he shuts his ears, locks up his understanding, because his grandfather and grandmother believed that the open-mouthed chimney was the one and only true English means of ventilation.

But suppose we were to say, "You love a cheerful blaze, can afford to pay for it, and therefore care not how much coal you waste in obtaining it. We also love a cheerful blaze, but have a great aversion to coal-smoke and tarry vapours; and we find that we can make a beautiful fire, quite inoffensive even in the middle of the room, provided we feed it with stale quartern loaves. We know that such fuel is expensive, but can afford to pay for it, and choose to do so." Would he not be shocked at the sight of the blazing loaves, if this extravagance were carried out?

This popular inconsistency of disregarding the waste of a valuable and necessary commodity, of which the supply is limited and absolutely unrenewable, while we have such proper horror of wilfully wasting another similar commodity which can be annually replaced as long as man remains in living contact with the earth, will gradually pass away when rational attention is directed to the subject. If the recent very mild suggestion of a coal-famine does something towards placing coal on a similar pedestal of popular veneration to that which is held by the "staff of life," the million a week that it has cost the coal consumer will have been profitably invested.

Many who were formerly deaf to the exhortations of fuel economists are now beginning to listen. "*Forty shillings per ton*" has acted like an incantation upon the spirit of Count Rumford. After an oblivion of more than 80 years,

his practical lessons have again sprung up among us. Some are already inquiring how he managed to roast 112 lbs. of beef at the Foundling Hospital with 22 lbs. of coal, and to use the residual heat for cooking the potatoes, and why it is that with all our boasted progress we do not now, in the latter third of the nineteenth century, repeat that which he did in the eighteenth.

The fact that the consumption of coal in London during the first four months of 1873 has, in spite of increasing population, amounted to 49,707 tons less than the corresponding period of 1872, shows that some feeble attempts have been made to economise the domestic consumption of fuel. One very useful result of the recent scarcity of coal has been the awakening of a considerable amount of general interest in the work of stock-taking, a tedious process which improvident people are too apt to shirk, but which is quite indispensable to sound business proceedings either of individuals or nations.

There are many discrepancies in the estimates that have been made of the total available quantity of British coal. The speculative nature of some of the data renders this inevitable, but all authorities appear to agree on one point, viz., that the amount of our supplies will not be determined by the actual total quantity of coal under our feet, but by the possibilities of reaching it. This is doubtless correct, but how will these possibilities be limited, and what is the extent or range of the limit? On both these points I venture to disagree with the eminent men who have so ably discussed this question. First, as regards the nature of the limit or barrier that will stop our further progress in coal-getting. This is generally stated to be the depth of the seams. The Royal Commissioners of 1870 base their tables of the quantity of available coal in the visible and concealed coal-fields upon the assumption that 4000 feet is the limit of possible working. This limit is the same that was taken by Mr. Hull ten years earlier. Mr. Hull, in the last edition of "The Coal Fields of Great Britain," p. 326, referring to Professor Ramsey's estimate, says, "These estimates are drawn up for depths down to 4000 feet below the surface, and even beyond this limit; but with this latter quantity it is scarcely necessary that we should concern ourselves." I shall presently show reasons for believing that the time may ultimately arrive when we *shall* concern ourselves with this deep coal, and actually get it; while, on the other hand, that remote epoch will be preceded by another period of practical approximate exhaustion

of British coal supply, which is likely to arrive long before we reach a working depth of 4000 feet.

The Royal Commissioners estimate that within the limits of 4000 feet we have hundreds of square miles of attainable coal capable of yielding, after deducting 40 per cent. for loss in getting, &c., 146,480 millions of tons; or, if we take this with Mr. Hull's deduction of one-twentieth for seams under two feet in thickness, there remains 139,000 millions of tons, which, at present rate of consumption, would last about 1200 years. But the rate of consumption is annually increasing, not merely on account of increasing population, but also from the fact that mechanical inventions are perpetually superseding hand labour, and the source of power in such cases is usually derived from coal. This consideration induced Professor Jevons, in 1865, to estimate that between 1861 and 1871 the consumption would increase from 83,500,000 tons to 118,000,000 tons. Mr. Hunt's official return for 1871 shows that this estimate was a close approximation to the truth, the actual total for 1871 having been 117,352,028 tons. At this rate of an arithmetical increase of three and a half tons per annum, 139,000 millions of tons would last but 250 years. Mr. Hull, taking the actual increase at three millions of tons per annum, extends it to 276 years. Hitherto the annual increase has followed a geometrical rather than arithmetical progress, and those who anticipate a continuance of this allow us a much shorter lease of our coal treasures. Mr. Price Williams maintains that the increase will proceed in a diminishing ratio like that of the increase of population; and upon this basis he has calculated that the annual consumption will amount to 274 millions of tons a hundred years hence, and the whole available stock of coal will last about 360 years.

The latest returns show, for 1872, an output of 123,546,758 tons, which, compared with 1871, gives a rate of increase of more than double the estimate of Mr. Hull, and indicate that prices have not yet risen sufficiently to check the geometrical rate of increase. Mr. Hull very justly points out the omission in those estimates which do not "take into account the diminishing ratio at which coal must be consumed when it becomes scarcer and more expensive;" but, on the other hand, he omits the opposite influence of increasing prices on production, which has been strikingly illustrated by the extraordinary number of new coal-mining enterprises that have been launched during the last six months. If we continue as we are now proceeding, a practical and permanent coal famine will be upon us within

the lifetime of many of the present generation. By such a famine, I do not mean an actual exhaustion of our coal seams (which will never be effected), but such a scarcity and rise of prices as shall annihilate the most voracious of our coal-consuming industries, those which depend upon abundance of cheap coal, such as the manufacture of pig-iron, &c.

The action of increasing prices has been but lightly considered hitherto, though its importance is paramount in determining the limits of our coal supply; I even venture so far as to affirm that it is not the depth of the coal seams, not the increasing temperature nor pressure as we proceed downwards, nor even thinness of seam, that will practically determine the limits of British coal-getting, but simply the price per ton at the pit's mouth.

In proof of this, I may appeal to actual practice. Mr. Hull and others have estimated the working limit of thinness at two feet, and agree in regarding thinner seams than this as unattainable. This is unquestionably correct so long as the getting is effected in the usual manner. A collier cannot lie down and hew a much thinner seam than this, if he works as colliers work at present. But the lead and copper miners succeed in working far thinner lodes, even down to the thickness of a few inches, and the gold-digger crushes the hardest component of the earth's crust to obtain barely visible grains of the precious metal. This extension of effort is entirely determined by market value. At a sufficiently high price the two feet limit of coal-getting would vanish, and the collier would work after the manner of the lead-miner.

We may safely apply the same reasoning to the limits of depth. The 4000 feet limit of the Royal Commissioners is *at present* unattainable, simply because the immediately prospective price of coal would not cover the cost of such deep sinking and working: but as prices go up, pits will go down, deeper and deeper still.

The obstacles which are assumed to determine the 4000 feet limit are increasing density due to greater pressure, and the elevation of temperature which proceeds as we go downwards. The first of these difficulties has, I suspect, been very much overstated, if not altogether misunderstood; though it is but fair to add that Mr. Hull, who most prominently dwells upon it, does so with all just and philosophic caution. He says that "it is impossible to speak with certainty of the effect of the accumulative weight of 3000 or 4000 feet of strata on mining operations. In all probability

one effect would be to increase the density of the coal itself, and of its accompanying strata, so as to increase the difficulty of excavating," and he concludes by stating that " In the face of these two obstacles—temperature and pressure, ever increasing with the depth—I have considered it utopian to include in calculations having reference to coal supply any quantity, however considerable, which lies at a greater depth than 4000 feet. Beyond that depth, I do not believe that it will be found practicable to penetrate. Nature rises up, and presents insurmountable barriers."*

On one point I differ entirely from Mr. Hull, viz., the conclusion that the increased " density of the coal itself and of its accompanying strata " will offer any serious obstacle. On the contrary, there is good reason to believe that such density is one of the essential conditions for working deep coal. Even at present depths of working, density and hardness of the accompanying strata is one of the most important conditions of easy and cheap coal-getting. With a dense roof and floor the collier works vigorously and fearlessly ; and he escapes the serious cost of timbering. Those who have never been underground, and only read of colliery disasters, commonly regard the fire-damp and choke-damp as the collier's most deadly enemies, but the collier himself has quite as much dread of a rotten roof as of either of these ; he knows by sad experience how much bruising, and maiming, and crushing of human limbs are due to the friability of the rock above his head. Mr. Hull quotes the case of the Dunkinfield colliery, where, at a depth of about 2500 feet, the pressure is " so resistless as to crush in circular arches of brick four feet thick," and to snap a cast-iron pillar in twain ; but he does not give any account of the density of the accompanying strata at the place of these occurrences. I suspect that it was simply *a want of density* that allowed the superincumbent pressure to do such mischief. The circular arches of brick four feet thick were but poor substitutes for a roof of solid rock of 40 or 400 feet in thickness ; an arch cut in such a rock would be all key-stone : and I may safely venture to affirm that if, in the deep sinkings of the future, we do encounter the increased density which Mr. Hull anticipates, this will be altogether advantageous. I fear, however, that it will not be so, that the chief difficulty of deep coal mining will arise from occasional " running in " due to deficient density, and that this difficulty will occur in about the same

* The Coal Fields of Great Britain, pp. 447 and 448.

proportion of cases as at present, but will operate more seriously at the greater depths.

A very interesting subject for investigation is hereby suggested. Do rocks of given composition and formation increase in density as they dip downwards, and if so, does this increase of density follow any law by which we may determine whether their power of resisting superincumbent pressure increases in any approach to the ratio of the increasing pressure to which they are naturally subjected? If the increasing density and power of resistance reaches or exceeds this ratio, deep mining has nothing to fear from pressure. If they fall short of it, the difficulties arising from pressure may be serious. Friability, viscosity, and power of resisting a crushing strain must be considered in reference to this question.

Mr. Hull has collected a considerable amount of data bearing upon the rate of increase of temperature with depth. His conclusions give a greater rate of increase than is generally stated by geologists; but for the present argument I will accept, without prejudice, as the lawyers say, his basis of a range of 1° F. for 60 feet. According to this, the rocks will reach 99·6°, a little above blood-heat, at 3000 feet, and 116·3° at the supposed limit of 4000 feet. It is assumed by Mr. Hull, by the Commissioners, and most other authorities, that this rock temperature of 116° will limit the possibilities of coal-mining. At the average prices of the last three years, or the prospective prices of the next three years, this temperature may be, like difficulties of the thin seams, an insurmountable barrier; but I contend that at higher prices we may work coal at this, and even far higher, rock temperatures; that it matters not how high the thermometer rises as we descend, we shall still go lower and still get coal so long as prices rise with the mercury. Given this condition, and I have no doubt that coal may be worked where the rock temperature shall reach or even exceed 212°. I do not say that we shall actually work coal at such depths; but if we do not, the reason will be, not that the thermometer is too high, but that prices are too low: in other words, value, not temperature, will determine the working limits.

Mr. Leifchild, in the last number of the "Edinburgh Review," in discussing this question, tells us that "the normal heat of our blood is 98°, and fever heat commences at 100°, and the extreme limit of fever heat may be taken at 112°. Dr. Thudicum, a physician who has specially investigated this subject, has concluded from experiments on his own body at

high temperatures, that at a heat of 140° no work whatever could be carried on, and that at a temperature of from 130° to 140° only a very small amount of labour, and that at short periods, was practicable; and further, that human labour daily and at ordinary periods, is limited by 100° of temperature, as a fixed point, and then the air must be dry, for in moist air he did not think men could endure ordinary labour at a temperature exceeding 90°."

It may be presumptuous on my part to dispute the conclusions of a physician on such a subject, but I do so nevertheless, especially as the data required are simple practical facts such as are better obtained by furnace-working than by sick-room experience.

During the hottest days of the summer of 1868, I was engaged in making some experiments in the re-heating furnaces at Sir John Brown and Co.'s works, Sheffield, and carried a thermometer about with me which I suspended in various places where the men were working. At the place where I was chiefly engaged (a corner between two sets of furnaces), the thermometer, suspended in a position where it was not affected by direct radiations from the open furnaces, stood at 120° while the furnace doors were shut. The *radiant* heat to which the men themselves were exposed while making their greatest efforts in placing and removing the piles was far higher than this, but I cannot state it, not having placed the thermometer in the position of the men. In one of the Bessemer pits the thermometer reached 140°, and men worked there at a kind of labour demanding great muscular effort. It is true that during this same week the puddlers were compelled to leave their work; but the tremendous amount of concentrated exertion demanded of the puddler in front of a furnace, which, during the time of removing the balls, radiates a degree of heat quite sufficient to roast a sirloin of beef if placed in the position of the puddler's hands, is beyond comparison with that which would be demanded of a collier working even at a depth giving a theoretical rock temperature of 212°, and aided by the coal-cutting and other machinery that sufficiently high prices would readily command. In some of the operations of glass-making, the ordinary summer working temperature is considerably above 100°, and the radiant heat to which the workmen are subjected far exceeds 212°. This is the case during a "pot setting," and in the ordinary work of flashing crown glass.

As regards the mere endurance of a high temperature, the

well-known experiments of Blagden, Sir Joseph Banks, and others have shown that the human body can endure for short periods a temperature of 260° F., and upwards. My own experience of furnace-work, and of Turkish baths, quite satisfies me that I could do a fair day's work of six or eight hours in a temperature of 130° F., provided I were free from the encumbrances of clothing, and had access to abundance of tepid water. This in a still atmosphere, but with a moving current of dry air capable of promoting vigorous evaporation from the skin, I suspect that the temperature might be ten or fifteen degrees higher. I *enjoy* ordinary walking exercise in a well-ventilated Turkish bath at 150°, and can endure it at 180°.

In order to obtain further information on this point, I have written to Mr. Tyndall, the proprietor of the Turkish baths at Newington Butts. He is an architect, who has had considerable experience in the employment of workmen and in the construction of Turkish baths and other hot air chambers. He says: "Shampooers work in my establishment from four to five hours at a time *in a moist atmosphere* at a temperature ranging from 105° to 110°. I have myself worked twenty hours out of twenty-four in one day in a temperature over 110°. Once for one half hour I shampooed in 185°. At the enamel works, in Pimlico, belonging to Mr. Mackenzie, men work daily in a heat of over 300°. The moment a man working in a 110° heat begins to drink alcohol, his tongue gets parched, and he is obliged to continue drinking while at work, and the brain gets so excited that he cannot do half the amount. I painted my skylights, taking me about four hours, at a temperature of about 145°; also the hottest room skylights, which took me one hour, coming out at intervals for a cooler, at a temperature of 180°. I may add in conclusion, that a man can work well in a moist temperature of 110° if he perspires freely."

The following, by a writer whose testimony may be safely accepted, is extracted from an account of ordinary passenger ships of the Red Sea, in the "Illustrated News," of November 9, 1872: "The temperature in the stoke-hole was 145°. The floor of this warm region is close to the ship's keel, so it is very far below. There are twelve boilers, six on each side, each with a blazing furnace, which has to be opened at regular intervals to put in new coals, or to be poked up with long iron rods. This is the duty of the poor wretches who are doomed to this work. It is hard to believe that human beings could be got to labour under

such conditions, yet such persons are to be found. The work of stoking or feeding the fires is usually done by Arabs, while the work of bringing the coal from the bunkers is done by sidi-wallahs or negroes. At times some of the more intelligent of these *are promoted to the stoking*. The negroes who do this kind of work come from Zanzibar. They are generally short men, with strong limbs, round bullet heads, and the very best of good nature in their dispositions. Some of them will work half an hour in such a place as the stoke-hole without a drop of perspiration on their dark skins. Others, particularly the Arabs, when it is so hot as it often is in the Red Sea have to be carried up in a fainting condition, and are restored to animation by dashing buckets of water over them as they lie on deck."

It must be remembered that the theoretical temperature of 116° at 4000 feet, the 133° at 5000 feet, or the 150° at 6000 feet, are the temperatures of the undisturbed rock; that this rock is a bad conductor of heat, whose surface may be considerably cooled by radiation and convection; and therefore we are by no means to regard the rock temperature as that of the air of the roads and workings of the deep coal pits of the future. It is true that the Royal Commissioners have collected many facts showing that the actual difference between the face of the rocks of certain pits and the air passing through them is but small; but these data are not directly applicable to the question under consideration for the three following reasons:—

First. The comparisons are made between the temperature of the air and the actual temperature of the opened and already-cooled strata, while the question to be solved is the difference between the theoretical temperature of the unopened earth depths and that of the air in roads and workings to be opened through them.

Second. The cooling effect of ventilation must (as the Commissioners themselves state) increase in a ratio which "somewhat exceeds the ratio of the difference between the temperature of the air and that of the surrounding surface with which it is in contact." Thus, the lower we proceed the more and more effectively cooling must a given amount of ventilation become.

The third, and by far the most important, reason is, that in the deep mining of the future, special means will be devised and applied to the purpose of lowering the temperature of the workings, that as the descending efforts of the collier increase with ascending value of the coal, a new problem will be offered for solution, and the method of

working coal will be altered accordingly. In the cases quoted by the Commissioners, the few degrees of cooling were effected by a system of ventilation that was devised to meet the requirements of respiration, and not for the purpose of cooling the mine.

It would be very presumptuous for any one in 1873 to say how this special cooling will actually be effected, but I will nevertheless venture to indicate one or two principles which may be applied to the solution of the problem. First of all, it must be noted that very deep mines are usually dry; and there is good reason to believe that, before reaching the Commissioners' limit of 4000 feet, dry mining would be the common, and at and below 4000 feet the universal, case. At present we usually obtain coal from water-bearing strata, and all our arrangements are governed by this very serious contingency. With water removed, the whole system of coal-mining may be revolutionised, and thus the aspect of this problem of cooling the workings would become totally changed.

Those who are acquainted with the present practice of mining are aware that when an estate is taken, and about to be worked for coal, the first question to be decided is the dip of the measures, in order that the sinking may be made "on the deep" of the whole range. The pits are not sunk at that part of the range where, at first sight, the coal appears the most accessible, but, on the contrary, at the deepest part. It is then carried on to some depth below the coal seam which is to be worked in order to form a "sumpf" or receptacle, from which the water may be wound or pumped. The necessity for this in water-bearing strata is obvious enough. If the collier began at the shallowest portion of his range, and attempted to proceed downwards, he would be "drowned out" unless he worked as a coal-diver rather than a coal-miner. By sinking in the deep he works upwards, away from the water, which all drains down to the sumpf.

The modern practice is to sink "a pair of pits," *both on the deep,* and within a short distance of each other. The object of the second is ventilation. By contrivances, which I need not here detail, the air is made to descend one of the pits, "the downcast shaft," then to traverse the roads and workings wherein ventilation is required, and return by a reverse route to the "upcast shaft," by which it ascends to the surface.

Thus it will be seen that, whenever the temperature of the roads and workings exceeds that of the outer atmosphere, the air currents have to be forced to travel through

the mine in a direction contrary to their natural course. The cooler air of the downcast shaft has to climb the inclined roads, and then after attaining its maximum temperature in the fresh workings must *descend* the roads till it reaches the upcast shaft. The cool air must rise and the warmer air descend.

What, then, would be the course of the mining engineer when all the existing difficulties presented by water-bearing strata should be removed, and their place taken by a new and totally different obstacle, viz., high temperature? Obviously to reverse the present mode of working—to sink on the upper part of the range and drive downwards. In such a system of working the ventilation of the pit will be most powerfully aided, or altogether effected, by natural atmospheric currents. An upcast once determined by artificial means, it will thereafter proceed spontaneously, as the cold air of the downcast shaft will travel by a descending road to the workings, and then after becoming heated will simply obey the superior pressure of the heavy column behind, and proceed by an upward road to the upcast shaft. As the impelling force of the air current will be the difference between the weight of the cool column of air in the downcast shaft and roads and the warm column in the upcast, the available force of natural ventilation and cooling will increase just as demanded, *i.e.*, it will increase with the depth of the workings and the heat of the rocks. A mining engineer who knows what is actually done with present arrangements, will see at once that with the above-stated advantages a gale of wind or even a hurricane might be directed through any partictular roads or long-wall workings that were once opened. Let us suppose the depth to be 5000 feet, the rock temperature at starting 133°, and that of the outer air 60°, we should have a torrent of air 73° cooler than the rocks rushing furiously downwards, then past the face of the heated strata, and absorbing its heat to such an extent that the upcast shaft would pour forth a perpetual blast of hot air like a gigantic furnace chimney.

But this is not all; the heat and dryness of these deep workings of the future places at our disposal another and vastly more efficient cooling agency than even that of a hurricane of dry-air ventilation. In the first part of the sinking of the deep shafts the usual water-bearing strata would be encountered, and the ordinary means of "tubbing" or "coffering" would probably be adopted for temporary convenience during sinking. Doorways, however, would be left in the tubbing at suitable places for tapping at pleasure the wettest and

most porous of the strata. Streams of cold water could thus be poured down the sides of the shaft, which, on reaching the bottom would flow by a downhill road into the workings. The stream of air rushing by the same route and becoming heated in its course would powerfully assist the evaporation of the water. The deeper and hotter the pit, the more powerful would be these cooling agencies.

As the specific heat of water is about five times that of the coal-measure rocks, or the coal itself, every degree of heat communicated to each pound of water would abstract one degree from five pounds of rock. But in the conversion of water at 60° into vapour at say 100°, the amount of heat absorbed is equivalent to that required to raise the same weight of water about 1000°, and thus the effective cooling power on the rock would be equivalent to 5000°.

The workings once opened (I assume as a matter of course that by this time pillar-and-stall working will be entirely abandoned for long-wall or something better), there would be no difficulty in thus pouring streams of water and torrents of air through the workings during the night, or at any suitable time preparatory to the operations of the miner, who long before the era of such deep workings will be merely the director of coal cutting and loading machinery.

Given a sufficiently high price for coal at the pit's mouth to pay wages and supply the necessary fixed capital, I see no insuperable difficulty, so far as mere temperature is concerned, in working coal at double the depth of the Royal Commissioners' limit of possibility. At such a depth of 8000 feet the theoretical rock-temperature is 183°.

By the means above indicated, I have no doubt that this could be reduced to an *air* temperature below 110°,—that at which Mr. Tyndall's shampooers ordinarily work. Of course the newly-exposed face of the coal would have its initial temperature of 183°; but this is a trivial heat compared to the red-hot radiant surfaces to which puddlers, shinglers, glassmakers, &c., are commonly exposed. Divested of the incumbrance of clothing, with the whole surface of the skin continuously fanned by a powerful stream of air—which, during working hours need be but partly saturated with vapour—a sturdy midland or north-countryman would work merrily enough at short hours and high wages, even though the newly-exposed face of the coal reached 212°; for we must remember that this new coal-face would only correspond to the incomparably hotter furnace-doors and fires of the steam-ship stoke holes.

The high temperature at 8000 or even 10,000 feet would present a really serious difficulty during the first opening of communication between the two pits. A spurt of brave effort would here be necessary, and if anybody doubts whether Englishmen could be found to make the effort, let him witness a "pot-setting" at a glass-house. Negro labour might be obtained if required, but my experience among English workmen leads me to believe that they will never allow negroes or any others to beat them at home in any kind of work, where the wages paid are proportionate to the effort demanded.

If I am right in the above estimates of working possibilities, our coal resources may be increased by about forty thousand millions of tons beyond the estimate of the Commissioners. To obtain such an additional quantity will certainly be worth an effort, and unless we suffer a far worse calamity than the loss of all our minerals, viz., a deterioration of British energy, the effort will assuredly be made.

I have said repeatedly that it is not physical difficulties, but market value, that will determine the limits of our coal mining. This, like all other values, is of course determined by the relation between demand and supply. Fuel being one of the absolute necessaries of life, the demand for it must continue so long as the conditions of human existence remain as at present, and the outer limits of the possible value of coal will be determined by that of the next cheapest kind of fuel which is capable of superseding it.

We begin by working the best and most accessible seams, and while those remain abundant the average value of coal will be determined by the cost of producing it under these easy conditions. Directly these most accessible seams cease to supply the whole demand, the market value rises until it becomes sufficient to cover the cost of working the less accessible; and now the average value will be regulated not by the cost of working what remains of the first or easy mines, but by that of working the most difficult that must be worked in order to meet the demand. This is a simple case falling under the well-established economic law, that the natural or cost value of any commodity is determined by the cost value of the most costly portion of it. Thus, the only condition under which we can proceed to sink deeper and deeper, is a demand of sufficient energy to keep pace with the continually increasing cost of production. This condition can only be fulfilled when there is no competing source of cheaper production which is adequate to supply the demand.

The question then resolves itself to this. Is any source of supply likely to intervene that will prevent the value of coal from rising sufficiently to cover the cost of working the coal seams of 4000 feet and greater depth? Without entering upon the question of peat and wood fuel, both of which will for some uses undoubtedly come into competition with British coal as it rises in value, I believe that there are sound reasons for concluding that our London fire-places, and those of other towns situated on the sea coast and the banks of navigable rivers, will be supplied with transatlantic coal long before we reach the Commissioners' limit of 4000 feet. The highest prices of last winter, if steadily maintained, would be sufficient to bring about this important change. Temporary upward jerks of the price of coal has very little immediate effect upon supply, as the surveying, conveying, boring, sinking, and fully opening of a new coal estate is a work of some years.

The Royal Commissioners estimate that the North-American coal-fields contain an untouched coal area equal to 70 times the whole of ours. Further investigation is likely to increase rather than diminish this estimate. An important portion of this vast source of supply is well situated for shipment, and may be easily worked at little cost. Hitherto, the American coal-fields have been greatly neglected, partly on account of the temptations to agricultural occupation which is afforded by the vast area of the American continent, and partly by the barbarous barriers of American politics. Large quantities of capital which, under the social operation of the laws of natural selection, would have been devoted to the unfolding of the vast mineral resources of the United States, are still wastefully invested in the maintenance of protectively nursed and sickly imitations of English manufactures. When the political civilisation of the United States becomes sufficiently advanced to establish a national free-trade policy, this perverted capital will flow into its natural channels, and the citizens of the States will be supplied with the more highly elaborated industrial products at a cheaper rate than at present, by obtaining them in exchange for their superabundant raw material from those European countries where population is overflowing the raw material supplies. When this time arrives, and it may come with the characteristic suddenness of American changes, the question of American *versus* English coal in the English markets will reduce itself to one of horizontal *versus* vertical difficulties. If at some future period the average depth of the Newcastle

coal pits becomes 3000 feet greater than those of the pits near the coast of the Atlantic or American lakes, and if the horizontal difficulties of 3000 miles of distance are less than the vertical difficulties of 3000 feet of depth, then coals will be carried from America to Newcastle. They will reach London and the towns on the South Coast before this, that is, when the vertical difficulties at Newcastle plus those of horizontal traction from Newcastle to the south, exceed those of eastward traction across the Atlantic.

As the cost of carriage increases in a far smaller ratio than the open ocean distance, there is good reason for concluding that the day when London houses will be warmed by American coal is not very far distant. We, in England, who have outgrown the pernicious folly of "protecting native industry," will heartily welcome so desirable a consummation. It will render unnecessary any further inquiry into the existence of London "coal rings" or combinations for restricted output among colliers or their employers. If any morbid impediments to the free action of the coal trade do exist, the stimulating and purgative influence of foreign competition will rapidly restore the trade to a healthy condition.

The effect of such introduction of American coal will not be to perpetually lock up our deep coal nor even to stop our gradual progress towards it. We shall merely proceed downwards at a much slower rate, for in America, as with ourselves, the easily accessible coal will be first worked, and as that becomes exhausted, the deeper, more remote, thinner, and inferior will only remain to be worked at continually increasing cost. When both our own and foreign coal cost more than peat, or wood, or other fuel, then and therefore will coal become quite inaccessible to us, and this will probably be the case long before we are stopped by the physical obstacles of depth, density, or high temperature.

As this rise of value must of necessity be gradual, and the superseding of British by foreign coal, as well as the final disuse of coal, will gradually converge from the circumference towards the centres of supply, from places distant from coal pits to those close around them, we shall have ample warning and opportunity for preparing for the social changes that the loss of the raw material will enforce.

The above-quoted writer, in the "Edinburgh Review," expresses in strong and unqualified terms an idea that is very prevalent in England and abroad: he says that "The course of manufacturing supremacy of wealth and of power is

directed by coal. That wonderful mineral, of the possession of which Englishmen have thought so little, but wasted so much, is the modern realisation of the philosopher's stone. This chemical result of primeval vegetation has been the means by its abundance of raising this country to an unprecedented height of prosperity, and its deficiency might have the effect of lowering it to slow decline."

* * "It raises up one people and casts down another; it makes railways on land and paths on the sea. It founds cities, it rules nations, it changes the course of empires."

The fallacy of these customary attributions of social potency to mere mineral matter is amply shown by facts that are previously stated by the reviewer himself. He tells us that "the coal fields of China extend over an area of 400,000 square miles; and a good geologist, Baron Von Richthofen, has reported that he himself has found a coal-field in the province of Hunau covering an area of 21,700 square miles, which is nearly double our British coal area of 12,000 square miles. In the province of Shansi, the Baron discovered nearly 30,000 square miles of coal with unrivalled facilities for mining. But all these vast coal fields, capable of supplying the whole world for some thousands of years to come, are lying unworked."

If "the course of manufacturing supremacy of wealth and of power" were directed by coal, then China, which possesses 33·3 times more of this directive force than Great Britain, and had had so early a start in life, should be the supreme summit of the industrial world. If this solid hydrocarbon "raises up one people and casts down another," the Chinaman should be raised thirty-three times and three-tenths higher than the Englishman; if it "makes railways on land and paths on the sea," the Chinese railways should be 33·3 times longer than ours, and the tonnage of their mercantile marine 33·3 times greater.

Every addition to our knowledge of the mineral resources of other parts of the world carries us nearer and nearer to the conclusion that the old idea of the superlative abundance of the natural mineral resources of England is a delusion. We are gradually discovering that, with the one exception of tin-stone, we have but little if any more than an average supply of useful ores and mineral fuel. It is a curious fact, and one upon which we may profitably ponder, that the poorest and the worst iron ores that have ever been commercially reduced, are those of South Staffordshire and the Cleveland district, and these are the two greatest iron-making centres of the world. There are no ores of copper,

zinc, tin, nickel, or silver in the neighbourhood of Birmingham, nor any golden sands upon the banks of the Rea, yet this town is the hardware metropolis of the world, the fatherland of gilding and plating, and is rapidly becoming supreme in the highest art of gold and silver work.

These, and a multitude of other analogous facts, abundantly refute the idea that the native minerals, the natural fertility, the navigable rivers, or the convenient seaports, determine the industrial and commercial supremacy of nations. The moral forces exerted by the individual human molecules are the true components which determine the resulting force and direction of national progress. It is the industry and skill of our workmen, the self-denial, the enterprise, and organising ability of our capitalists, that has brought our coal so precociously to the surface and redirected for human advantage the buried energies of ancient sunbeams, while the fossil fuel of other lands has remained inert.

The foreigner who would see a sample of the source of British prosperity must not seek for it in a geological museum or among our subterranean rocks; let him rather stand on the Surrey side of London Bridge from 8 to 10 a.m. and contemplate the march of one of the battalions of our metropolitan industrial army, as it pours forth in unceasing stream from the railway stations towards the city. An analysis of the moral forces which produce the earnest faces and rapid steps of these rank and file and officers of commerce will reveal the true elements of British greatness, rather than any laboratory dissection of our coal or ironstone.

Fuel and steam-power have been urgently required by all mankind. Englishmen supplied these wants. Their urgency was primary and they were first supplied, even though the bowels of the earth had to be penetrated in order to obtain them. In the present exceptional and precocious degree of exhaustion of our coal treasures, we have the *effect* not the *cause* of British industrial success.

If in a ruder age our greater industrial energy enabled us to take the lead in supplying the ruder demands of our fellow-creatures, why should not a higher culture of those same abundant energies qualify us to maintain our position, and enable us to minister to the more refined and elaborate wants of a higher civilisation? There are other necessary occupations quite as desirable as coal-digging, furnace-feeding, and cotton-spinning.

The approaching exhaustion of our coal supplies should

therefore serve us as a warning for preparation. Britain will be forced to retire from the coal-trade, and should accordingly prepare her sons for higher branches of business, —for those in which scientific knowledge and artistic training will replace mere muscular strength and mechanical skill. We have attained our present material prosperity mainly by our excellence in the use of steam power; let us ever struggle for supremacy in the practical application of brain-power.

We have time and opportunity for this. The exhaustion of our coal supplies will go on at a continually retarding pace—we shall always be approaching the end, but shall never absolutely reach it, as every step of approximation will diminish the rate of approach; like the everlasting process of reaching a given point by continually halving our distance from it.

First of all we shall cease to export coal, then we shall throw up the most voracious of our coal-consuming industries, such as the reduction of iron ore in the blast-furnace; then copper smelting and the manufacture of malleable iron and steel from the pig, and so on progressively. If we keep in view the natural course and order of such progress, and intelligently prepare for it, the loss of our coal need not in the smallest degree retard the progress of our national prosperity.

If, however, we act upon the belief that the advancement of a nation depends upon the mere accidents of physical advantages, if we fold our arms and wait for Providence to supply us with a physical substitute for coal, we shall become Chinamen, minus the unworked coal of China.

If our educational efforts are conducted after the Chinese model; if we stultify the vigour and freshness of young brains by the weary, dull, and useless cramming of words and phrases; if we poison and pervert the growing intellect of British youth by feeding it upon the decayed carcases of dead languages and on effete and musty literature, our progress will be proportionally Chinaward; but if we shake off that monkish inheritance which leads so many of us blindly to believe that the business of education is to produce scholars rather than men, and direct our educational efforts towards the requirements of the future rather than by the traditions of the past, we need have no fear that Great Britain will decline with the exhaustion of her coal fields.

The teaching and training in schools and colleges must be directly and designedly preparatory to those of the workshop, the warehouse, and the office; for if our progress is

to be worthy of our beginning, the moral and intellectual dignity of industry must be formally acknowledged and systematically sustained and advanced. Hitherto, we have been the first and the foremost in utilising the fossil forces which the miner has unearthed; hereafter we must in like manner avail ourselves of the living forces the philosopher has revealed. Science must become as familiar among all classes of Englishmen as their household fuel. The youth of England must be trained to observe, generalise, and *investigate* the phenomena and forces of the world outside themselves; and also those moral forces within themselves, upon the right or wrong government of which the success or failure, the happiness or misery of their lives will depend.

With such teaching and training the future generations of England will make the best and most economical use of their coal while it lasts, and will still advance in material and moral prosperity in spite of its progressive exhaustion.

VII. ON THE INTRODUCTION OF GENERA AND SPECIES IN GEOLOGICAL TIME.*

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THERE can be no doubt that the theory of evolution, more especially that phase of it which is advocated by Darwin, has greatly extended its influence, especially among young English and American naturalists, within the few past years. We now constantly see reference made to these theories, as if they were established principles, applicable without question to the explanation of observed facts, while classifications notoriously based on these views, and in themselves untrue to nature, have gained currency in popular articles and even in text-books. In this way young people are being trained to be evolutionists without being aware of it, and will come to regard nature wholly through this medium. So strong is this tendency, more especially in England, that there is reason to fear that natural history will be prostituted to the service of a shallow philosophy, and that our old Baconian mode of viewing

* Forming portions of the President's Annual Address.

nature will be quite reversed, so that instead of studying facts in order to arrive at general principles, we shall return to the mediæval plan of setting up dogmas based on authority only, or on metaphysical considerations of the most flimsy character, and forcibly twisting nature into conformity with their requirements. Thus "advanced" views in science lend themselves to the destruction of science, and to a return to semi-barbarism.

In these circumstances, the only resource of the true naturalist is an appeal to the careful study of groups of animals and plants in their succession in geological time. I have myself endeavoured to apply this test in my recent report on the Devonian and Silurian flora of Canada, and have shown that the succession of Devonian and Carboniferous plants does not seem explicable on the theory of derivation. Still more recently, in a memoir on the Post-pliocene Deposits of Canada, now in course of publication in the "Canadian Naturalist," I have by a close and detailed comparison of the numerous species of shells found embedded in our clays and gravels, with those living in the Gulf of St. Lawrence and on the coasts of Labrador and Greenland, shown, that it is impossible to suppose that any changes of the nature of evolution were in progress; but, on the contrary, that all these species have remained the same, even in their varietal changes, from the Post-pliocene period until now. Thus the inference is that these species must have been introduced in some abrupt manner, and that their variations have been within narrow limits and not progressive. This is the more remarkable, since great changes of level and of climate have occurred, and many species have been obliged to change their geographical distribution, but have not been forced to vary more widely than in the Post-pliocene period itself.

Facts of this kind will attract little attention in comparison with the bold and attractive speculations of men who can launch their opinions from the vantage ground of London journals; but their gradual accumulation must some day sweep away the fabric of evolution, and restore our English science to the domain of common sense and sound induction. Fortunately, also, there are workers in this field beyond the limits of the English-speaking world. As an eminent example we may refer to Joachim Barrande, the illustrious palæontologist of Bohemia, and the greatest authority on the wonderful fauna of his own primordial rocks. In his recent memoir on those ancient and curious crustaceans, the Trilobites, published in advance of the

supplement to vol. i. of the Silurian system of Bohemia, he deals a most damaging blow at the theory of evolution, showing conclusively that no such progressive development is reconcileable with the facts presented by the primordial fauna. The Trilobites are very well adapted to such an investigation. They constitute a well marked group of animals trenchantly separated from all others. They extend through the whole enormous length of the Palæozoic period, and are represented by numerous genera and species. They ceased altogether at an early period of the earth's geological history, so that their account with nature has been closed, and we are in a condition to sum it up and strike the balance of profit and loss. Barrande, in an elaborate essay of 282 pages, brings to bear on the history of these creatures his whole vast stores of information in a manner most conclusive in its refutation of theories of progressive development.

It would be impossible here to give an adequate summary of his facts and reasoning. A mere example must suffice. In the earlier part of the memoir he takes up the modifications of the head, the thorax, and the pygidium or tail piece of the Trilobites in geological time, showing that numerous and remarkable as these modifications are, in structure, in form, and in ornamentation, no law of development can be traced in them. For example, in the number of segments or joints of the thorax, we find some Trilobites with only one to four segments, others with as many as fourteen to twenty-six, while a great many species have medium or intervening numbers. Now in the early primordial fauna the prevalent Trilobites are at the extremes, some with very few segments, as *Agnostus,* others with very many, as *Paradoxides.* The genera with the medium segments are more characteristic of the later faunas. There is thus no progression. If the evolutionist holds that the few-jointed forms are embryonic, or more like to the young of the others, then on his theory they should have precedence, but they are contemporary with forms having the greatest number of joints, and Barrande shows that these last cannot be held to be less perfect than those with the medium numbers. Further, as Barrande well shows, on the principle of survival of the fittest, the species with the medium number of joints are best fitted for the struggle of existence. But in that case the primordial Trilobites made a great mistake in passing at once from the few to the many segmented stage, or *vice versa,* and omitting the really profitable condition which lay between. In subsequent times they were thus obliged

to undergo a retrograde evolution, in order to repair the error caused by the want of foresight or precipitation of their earlier days. But, like other cases of late repentance, theirs seems not to have quite repaired the evils incurred; for it was after they had fully attained the golden mean that they failed in the struggle, and finally became extinct. "Thus the infallibility which these theories attribute to all the acts of matter organising itself is gravely compromised," and this attribute would appear not to reside in the trilobed tail any more than according to some in the triple crown.

In the same manner, the palæontologist of Bohemia passes in review all the parts of the Trilobites, the succession of their species and genera in time, the parallel between them and the Cephalopods, and the relations of all this to the primordial fauna generally. Everywhere he meets with the same result; namely, that the appearance of new forms is sudden and unaccountable, and that there is no indication of a regular progression by derivation. He closes with the following somewhat satirical comparison, of which I give a free translation:—"In the case of the planet Neptune, it appears that the theory of astronomy was wonderfully borne out by the actual facts as observed. This theory, therefore, is in harmony with the reality. On the contrary, we have seen that observation flatly contradicts all the indications of the theories of derivation with reference to the composition and first phases of the primordial fauna. In truth, the special study of each of the zoological elements of that fauna has shown that the anticipations of the theory are in complete discordance with the observed facts. These discordances are so complete and so marked that it almost seems as if they had been contrived on purpose to contradict all that these theories teach of the first appearance and primitive evolution of the forms of animal life."

This testimony is the more valuable, inasmuch as the annulose animals generally, and the Trilobites in particular, have recently been a favourite field for the speculations of our English evolutionists. The usual *argumentum ad ignorantiam* deduced from the imperfection of the geological record, will not avail against the facts cited by Barrande, unless it could be proved that we know the Trilobites only in the last stages of their decadence and that they existed as long before the Primordial as this is before the Permian. Even this supposition, extravagant as it appears, would by no means remove all the difficulties.

VIII. THE FUTURE OF THE ENGLISH LANGUAGE.*

By WILLIAM E. A. AXON, M.R.S.L., F.S.S.

A UNIVERSAL language has been the dream of many minds. It has been a subject of frequent aspiration, hope, and despair. That the civilised earth should speak one common dialect is indeed a "consummation devoutly to be wished." The number of languages in existence at the present moment is unknown, but, as Professor Müller has said, they cannot be less than 900. Adelung has estimated the number of known dialects at 3664, of which 937 belong to Asia, 587 to Europe, 276 to Africa, and 1624 to America. Balbi has enumerated 860 languages, forming about 5000 dialects. Of these languages 53 belong to Europe, 153 to Asia, 115 to Africa, 422 to America, and 117 to Oceania. There can be no doubt that this estimate very greatly underrates in every particular the number of existing methods of speech.

If we contemplate the amazing variety of this Babel of sounds, the first sentiment is one of wonder at the sanguine hopefulness of those who expect to see this chaos reduced to order and symmetry. Some, dismayed perhaps by the great number of dialects, have thought it impossible that any one language should ever conquer all its opponents, and remain in undisputed possession of the field, and have therefore sought for a method by which the same symbol should represent one idea and many sounds. That such a scheme is absolutely impossible would be too much to say, for a plan of this kind is already applied in the case of numerals. The figure 1 is called by the Italian *uno*, by the Welshman *un*, by the German *ein*; but to all three it conveys the idea of unity. The Frenchman's *quatre-vingt-douze* is very unlike in sound to the English ninety-two, but the figures 92 represent them both.† The construction of an artificial philosophical language, if not beyond the bounds of possibility, is too far from the realms of the practical to need more than passing mention, and the chances

* A considerable portion of this paper was originally delivered as a Presidential Address, April 3rd, 1873, before the Manchester Eclectic Society.

† By a false analogy Wachter, who saw that ten figures were sufficient for all calculations, was led to suppose that all writing might be managed by an alphabet of ten letters, and this is what he proposed:—

of its adoption even when created would be of the very smallest.*

A few centuries ago, the *learned* were really in possession of a universal language. Learning, confined then to a comparatively small number of individuals, was all consigned to the Latin language. In the street the scholar spoke his mother-tongue, but in the study and in the lecture-room Latin alone was heard. He wooed his sweetheart in English or in German, as the case might be; but he wooed the muses in the words which had served Virgil and Cicero. Many circumstances contributed to this result. Latin was the language of the church, and the literary class was for a long period, to a very large extent, made up of the priestly caste. It was not that all priests were literate, the reverse being, unhappily, often the case; but outside the clerical professions there was no place for the activity and learning of the student. And the most ignorant members of the priesthood would have at least some knowledge of the Latin tongue. Latin was the common universal language of the *literati* of Europe up to the period of the *Renaissance*. The Reformation shattered the unity of the western church, and led to the use in various countries of vernacular liturgies and translations of the Bible. The successive development

Genus.	Figura.	Potestas.
Vocal		a, e, i, o, u.
Guttur		k, c, ch, q, g, h.
Lingual		l.
Lingual		d, t.
Lingual		r.
Dental		s.
Labial		b, q.
Labial		m.
Labial		f, ph, v, w.
Nasal		n.

Supposing a language existed containing only ten sounds, they might be amply sufficient for the expression of ideas, since it has been estimated that they would form 3,628,800 combinations.—KOOPS on Paper. 2nd. ed., 1801, pp. 28 and 32.

* Bishop Wilkins's "Real Character" is hardly known now, except from Professor Müller's masterly analysis of it, in his "Science of Language" (vol. ii., p. 47). It was based upon a classification of the attributes of the subjects of knowledge. An idea of Wilkins's, founded on the analogy of the scientific symbols used in the European languages, has been developed into a system of ideographs by De Mas (*Ibid.*, p. 48).

of the rich popular literature of Italy, Spain, France, and our own country still further weakened it. Yet we see that, so late as the time of the English commonwealth, it was necessary to write in Latin for a European audience. Milton, when pleading for a free press in that republic, used eloquent and earnest English words; but when he had to defend the commonwealth against its foreign assailants, he used the Latin tongue. Salmasius attacked the English nation before the literary tribunal of Europe, and both plea and reply are in the language of the courts. A little earlier we have a still more striking instance in the case of Lord Bacon, all of whose most important writings were written in Latin. Fancy Darwin or Huxley thinking it necessary to their fame, and to the propagation of their theories, to write in any language but their own. When Newton's grand discoveries were made, they were recorded, not in English, but in Latin. Yet, when Bacon disdained to issue in English his views on the method of philosophy, it had received the plays of Shakespeare and the authorised version of the Scriptures, and in Newton's time it had been ennobled and dignified by the mighty music of Milton's verse.

Latin retained its hold upon the physical sciences long after it had ceased to be used to any great extent in any other field of literature. Even in this field it has now lost its position. There are very few works of any great scientific importance which have been issued in Latin during the past century. At present, of the writers on science, each one uses his own language, and leaves the propagation of his views to the mercy of translators, or the linguistic acquirements of his fellow-scholars. At no date were these probably greater than at present. The knowledge of languages has become a very common accomplishment; but, after all, the acquirement of foreign idioms is a difficult thing, and there must always be in every language a sort of holy of holies, into which the feet of the Gentile can never enter.* It is also obvious that the study necessary to

* A recent writer gives his own linguistic experiences:—"As a boy, we were taught Greek and Latin, such an amount as enabled us to read a Greek testament with the use occasionally of a lexicon, and to read freely Ovid and Virgil. But our future career was selected to be one in which Greek and Latin were not subjects for examination, but French and German 'paid well;' consequently, four years were devoted to the study of these two languages,—at the end of which time we found ourselves in South Africa, where the only languages of any practical use were Dutch and Caffre. To Dutch and Caffre, consequently, we turned our attention; and, after rather more than a year's study, we were able to converse imperfectly in both these. But again were we on the point of finding these later labours useless, for there was every prospect

master merely the most important of the living languages must detract considerably from the amount of time which can be applied to the enlarging of the bounds of science. Let us disabuse ourselves of the vulgar notion that the man of science is a sort of lucky guesser, who arrives at conclusions by process of conjuring. Let us remember that he must be first of all an instructed man, well acquainted with what has already been done, and what is actually being done. De Morgan speaks very emphatically on this point: "New knowledge, when to any purpose, must come by contemplation of old knowledge, in every matter which concerns thought; mechanical contrivance sometimes, not very often, escapes this rule. All the men who are now called discoverers, in every matter ruled by thought, have been men versed in the minds of their predecessors, and learned in what had been done before them. I may cite, among those who have wrought strongly upon opinion or practice in science, Aristotle, Plato, Ptolemy, Euclid, Archimedes, Roger Bacon, Copernicus, Francis Bacon, Ramus, Tycho Brahé, Galileo, Napier, Descartes, Liebnitz, Newton, Locke. I have taken none but names known out of their fields of work, and all were learned as well as sagacious."*

But at no previous period was there such a general diffusion of scientific investigation. The problems which engage the attention of the physicists of London and Berlin are also being eagerly scrutinised by those of Florence, Boston, Melbourne, and Cracow. That men should at the same time be accomplished linguists and profound scientists, is more than can be reasonably expected. There can, then, be

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of our services being transferred to India; and we heard, from good authority, that we were not likely to get on there unless we could speak Hindustani, and perhaps understood Sanscrit or Persian. Here, then, were Greek, Latin, French, German, Dutch, Caffre, Hindustani, Persian, Sanscrit, all to be learned, in order that one's own thoughts and wishes should be made intelligible to another person. In our judgment, this is not only a mistake, but it is a mistake which is remediable, and which is a slur upon the common-sense and civilisation of the world." After pointing out that in music there is but one language, he suggests that "a committee of the scientific men of all nations should be formed, which should decide on a language that shall be termed the universal language. Let us suppose that German be found to be the most expressive and complete of existing languages, and the one decided upon as the universal tongue. We commence our education, not with a superficial knowledge of several languages, but with a thorough knowledge of German only. All other nations adopt the same course; and we know that wherever civilisation has spread, wherever missionaries have resided and taught, we who speak this universal language shall be at once intelligible, and able to communicate our thoughts readily."—*Chambers's Journal*, January, 1872.

* Budget of Paradoxes, 1872, p. 4.

no doubt that this diversity of languages is an evil for science, since it puts serious difficulties in the way of the highest scientific culture, which consists, to use Dr. Matthew Arnold's phrase, in "acquainting ourselves with the best that has been known and said in the world" on the particular object of our study.

The advantage to commerce of a common language is so obvious that it needs only to be named in order to be appreciated. Is there any modern language which has any chance of becoming the general medium of civilised intercourse, both in speech and in writing? At one time the French language appeared likely to succeed to the heritage of the Latin. It was the language of diplomacy and of society; its affinity to Latin made it easy of acquisition to the Teutonic races who had learned Latin in their schools; and to the people of South Europe it was already three parts known from its analogies with their own vernaculars.* That day has passed. If any language ever becomes dominant, it is very unlikely that it will be French. France is no coloniser. She is great, but her boundaries are limited. Her home population decreases; her emigrants, instead of founding new Frances, are absorbed in the new Englands which are dotted over the globe.

The German is no more a national coloniser than the Frenchman. He increases much faster, but beyond the boundaries of the Fatherland the language makes small progress. The race goes to strengthen the American stock, but the language has no root in the American soil.

The best way to estimate the relative chances of various languages will be to ascertain the number of individuals who speak each of them. The statistics of language have not received a very large amount of attention, but the number of wide-extended languages is not very great. In this case we may safely leave out of consideration the languages which are not of European origin. The oriental tongues are not aggressive nor numerically strong enough to be factors in the problem. The materials for a rigidly accurate census of languages do not exist, but an approximately correct solution can be formed:—

PORTUGUESE.

In Portugal	3,980,000
,, Brazil	10,000,000
	13,980,000

* There was a time when the Academy of Berlin published its transactions n French.

ITALIAN.

In Italy	26,796,253
,, France	540,985
,, Switzerland	186,000
	27,524,238

Italy has a certain commercial currency in the Mediterranean, but has not taken root.

FRENCH.

In France	36,225,000
,, Belgium.	2,325,000
,, Switzerland	638,000
France has very few colonies. If all their populations spoke French, it would only add 3,631,000 persons. A million is a fair estimate	1,000,000
	40,188,000

RUSSIAN.

It has been said that there are 24 languages spoken in the Russian Empire, but the prevailing one is the Russ, and the number of those who speak it is reckoned at 51,370,000.

SPANISH.

Spain, including the Canary and Balearic Isles.	16,301,000
South America. If we give Spanish all the South American States except Brazil, there will be	27,408,082
	43,709,082

GERMAN.

German Empire	41,058,000
Austria	9,160,000
Belgium	2,747,000
Russia	985,000
Finland	1,000
Switzerland	1,838,000
	55,789,000

De Candolle has estimated the German-speaking peoples at 62,000,000, which appears too high a figure.*

* These figures are chiefly taken from the "Almanach de Gotha" for 1873, the conjectural estimates of the number of foreign-speaking people in each country being omitted. There *may be* fifty thousand Germans in Great Britain, and one thousand of them in Greece, but it is a matter of conjecture which does not affect the question we have in view.

ENGLISH.

English is spoken by 40,000,000 in the United States, by 50,000 in the republic of Liberia, by 31,000,000 British subjects in Europe, by 5,000,000 in America, by 2,000,000 in Australia, and by at least 1,000,000 more scattered over the various British dependencies in Asia and Africa, giving a grand total of 79,050,000.

From this it will be evident that English is at present the most widely spread of the languages of civilisation. But there is another point of importance which has been well put by M. de Candolle. Nations vary greatly as to the relative quickness with which they double themselves. He has worked out the problem, and has calculated the number of persons who will speak these languages in a century from now. Let us apply his method to figures of population, which sometimes vary from the estimates he has made, and see what will be the probable number of persons speaking the most important of the European languages at the end of the twentieth century.*

In England the population doubles itself in every 56 years; in the New World the Anglo-Saxons double in every 25 years. The Dutch double in 106 years; the Turks in 555 years; the Italians in 135 years; the Swedes in 92 years; the Russians in 100 years; the Spaniards in 112 years; their South-American descendants in 27½ years. This last was Humboldt's computation, and has been adopted here, although it may be doubted if this rate of increase has not been considerably checked by the chronic anarchy to which they are subject.† The North German people double in from 50 to 60 years, and the South Germans in 167 years, say 100 years as a mean for the entire race. The French populations take about 140 years in which to double.‡

* M. de Candolle's work ("Histoire des Sciences et des Savants depuis deux Siècles, suivie d'autres Etudes," par ALPHONSE DE CANDOLLE, Géneve, 1873), is one of great interest, alike from the subject-matters with which it deals and from the charm of style and treatment. In the essay with which we are more immediately concerned ("Avantage pour les Sciences d'une Langue Dominante et laquelle des Langues Modernes sera nécessairement Dominante au XXme Siècle"), he estimates that in 1970 there will be 860 million English-speaking persons to 124 million German, and 69½ million French-speaking persons.

† These estimates are derived from the following sources:—"Universal Language," by WILLIAM WHITE, p. 3; Bath, 1850. DE CANDOLLE: Hist. des Sciences. Encyclopædia Brit., art. "Population." Statistical Journal, vol. xxxiii.

‡ The following appears in the "Lancet," of May 3rd, 1873:—"M. Lagneau has placed before the Academy of Medicine of Paris a paper, from which it appears that the discouraging features presented by the quinquennial census

We may estimate on this basis that in the year 2000 the most important languages will be spoken by the number of persons as under :—

ITALIAN		53,370,000
FRENCH		72,571,000
RUSSIAN		130,479,800
GERMAN		157,480,000
SPANISH—		
Europe	36,938,338	
S. America	468,347,904	
		505,286,242
ENGLISH—		
Europe	178,846,153	
United States and non-European British dependencies . . .	1,658,440,000	
		1,837,286,153

From this it is tolerably clear that English is the language of the future. No other European tongue can compete with it, for no other race has the same wide field for extension. The emigrants who crowd to the West, be they Latin, Teutonic, or Scandinavian, become most surely and certainly Americanised. For a time they may endeavour to retain the language of their fatherland, but the attempt is hopeless. "In America," says Sir Charles Dilke, "the peoples of the world are being fused together, but they are run into an English mould; Alfred's laws and Chaucer's tongue are theirs, whether they would or no." In South

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which preceded the year 1866 are worse in the census of 1872. For the six years preceding 1872, the population has again decreased by 366,935 inhabitants—*i.e.*, 16 individuals upon every 10,000 (the 1,597,238 inhabitants lost with Alsace and Lorraine being deducted). In order to see the extent of the decrease, it should be remembered that the former census gave an annual increase of 38 per 10,000, so that the actual decrease is 54 per 10,000. M. Lagneau shows that this thinning of the population is owing to the comparative small number of births, military service and the anxiety of parents to prevent a great division of property being at the root of the evil. The author points out that, if this system continues, France in half a century could raise an army only one-fourth superior in number to the present, as the population would have increased only one-fourth; whilst in the same half-century, the number of inhabitants having doubled in England, Russia, and Germany, these countries could raise armies double the number of the present. He hopes, however, that a change will come over France, and that sufficient work and occupation will be found for the more numerous children of each family. He cites where the mortality is the same as in France, viz., 228 per 10,000, but where the births are 354, being one-third higher than in France; the annual increase, 126 per 10,000, three times higher than French increase, and where the population is doubled in 55 years, which doubling would in France take 183 years to effect."

America Spanish is the common language, and in Brazil 10,000,000 persons use the Portuguese; but neither of these have any propagandist power, and they will not improbably disappear before the more energetic English speech. The German-speaking peoples have no colonies or dependencies; those of France are unimportant; whilst those of Great Britain are scattered over every part of the globe. The British Empire covers nearly a third of the earth's surface, and British subjects are nearly a fourth of the population of the world. The native races of India, numbering 190,000,000 human beings, are governed by a mere handful of Englishmen; and it would be no new thing in the world's history if these subject races were to learn and adopt the language of their conquerors. That our language and literature are extensively cultivated by the educated natives already we know; but how long it may take before scholastic agencies reach the great mass of the people it is hard to say.

The widespread territorial influence of the British Empire must inevitably aid in extending the boundaries of the language, and another element of equal importance is the extent of our commercial intercourse with other nations, owing to the restless energy of our people, who are to be found wherever dash and endurance are needed. The adoption of the English language by the immense population of Japan has been seriously considered by the governors of that nation.

Such, then, is the position of the English language at the present day. It is spoken by a larger number of persons than any other civilised language, and those who speak it have proved themselves to be the most energetic, enterprising, and successful of modern races. The English race has fuller opportunities for further extension and development than any other. It is therefore of importance to ascertain if this language which has these external advantages possesses also the internal qualities necessary for the common language of civilisation. The civilisation and science of to-day are due mainly to the Latin and Teutonic races. The Sclavonic nations may have a great part to play in the future, but so far, their influence upon the literature and learning of the world has not been great. That language which is to be dominant must, as De Candolle has already said, have sufficient of Latin and German forms and words to show a genuine affinity with both those families of speech. Beyond this, it should be clear, simple, and brief.

A glance at the history of our language will show how

well it answers the first condition. To the strength of the Teutonic dialects it adds the clearness of the Latin, and a brevity that is all its own. A mixed language, it has combined the best elements of each. It is the language of men of business, to whom time is of importance, and who cannot afford to waste the stuff of which life is made, by roundabout phrases and ambiguous sentences. The object of those who have formed the English language might have been to see in how few words an idea could be conveyed. There is a directness of purpose about our most ordinary forms of expression. The question asked is not how can this thought be clothed in the most beautiful and appropriate diction, but how can it be rapidly and unmistakably expressed? It goes to the root of the matter, allows of no beating about the bush, but is exact, curt, pointed, and straightforward. English is not so long-winded as either French or German. De Candolle tells us that, in families where they have an equal acquaintance with French and with German, the former is always more used; and where English and French are spoken, the preference is given to English. German families, he says, settling in English or French countries quickly cease to use their own language, whilst Frenchmen and Englishmen settling in German countries are on the contrary very tenacious of their mother-tongue. It is possible to give another interpretation to these facts; but it seems not unnatural that those having choice of two roads should select the shortest and directest of them.

The English tongue has been the subject of many eulogies. Those which come from foreigners may at least claim sincerity and freedom from that national vanity which might induce an Englishman to over-estimate its beauty and importance. Jacob Grimm has said that "the English language possesses a power of expression such as was never, perhaps, attained by any human tongue. Its altogether intellectual and singularly happy foundation, and government, and development, has arisen from a surprising alliance between the two noblest languages of antiquity—the German and the Romanesque—the relations of which to each other is well known to be such that the former supplies the material foundation, and the latter the abstract notions. Yes; truly the English language may with good reason call itself a universal language, and seems chosen, like the English people, to rule in future times in a still greater degree in all corners of the earth. In richness, sound reason, and flexibility, no modern tongue can be

compared with it,—not even the German, which must shake off many a weakness before it can enter the lists with the English."*

The great defect of our language is its absurd orthography. This is the stumbling-block which prevents the ready acquisition of the spoken language by foreigners, and hinders the majority of our own people from acquiring an intelligent acquaintance with the riches of our literature. M. de Candolle was surprised to see that intelligent English children learned to read with great difficulty. He found the reason to be that each letter has many sounds, or that each sound is written in many different ways. "They are obliged to learn word by word. It is a matter of memory, almost entirely destitute of rule." The great defect of our language in the eyes of this critic, who is certainly not an adverse one, "is an orthography entirely irregular, so absurd that it requires more than a year for children to learn to read in it." More than a year! The hindrance which it causes to elementary education is much greater than this.

Mr. Russell Martineau, in a report to the Philological Society, says:—"How spelling can be taught at all in elementary schools is a constant wonder to me. There is not a single rule which a teacher can lay down which has not almost as many exceptions as examples. Thus—'Final *e* lengthens the preceding vowel, as in make, bite;' but then, what of love, glove, tongue? '*G* before *e* or *i* is sounded like *j*, as in gentle, gin;' but gig, gild, get protest. '*Gh* after *au* and *ou* is sounded like *f*, as laugh, cough, rough;' but what of haughty, plough, bough? And, worst of all, what can the teacher make of the double vowels *ea* in each, bread, great; *ai* in hail, against; *au* in fault, haunch, laugh; *ou* in sound, wound, soul; *ow* in blow, trowel; *ew* in yew, shew; *ei* in receive, reign; *ie* in field, tie, friend? Or, approaching the subject from the other side, the following vowel sounds have a plurality of modes of expression, between which the luckless pupil has to choose:—

a in ale	a, ai, ay, ea.
e ,, eel	e, ee, ea, ei, ie.
e ,, ell	e, ea, ai.
i ,, idle	i, ie, ei.
o ,, old	o, oe, ou, ow, ew, oa.
u ,, cue	u, ue, ew.
ou ,, pound	ou, ow.
au ,, fault	a, au, aw.

* Quoted by Mr. Pitman, in his "Manual of Phonography," 1873, page 4.

"I am not speaking too strongly in saying that our want of systematic orthography has reduced the advantage of alphabetic writing to a minimum, and makes correct spelling virtually impossible."

"It is the universal testimony of teachers," remarks Mr. E. Jones, B.A., Head Master of the Hibernian Schools, Liverpool, "that the irregularity of our spelling is a serious obstruction to education. *The bulk of the children pass through the government schools without having acquired the ability to read with ease and intelligence, or to spell with accuracy,* although these subjects, with arithmetic, occupy most of the time in these schools. It takes from six to seven years to learn the arts of reading and spelling with a fair degree of intelligence, and to many minds the difficulties of orthography are insurmountable. The report of the Birmingham Education Aid Society shows that, after a careful examination of a large number of youths of both sexes, between the ages of thirteen and twenty, employed in the factories in that town, only four and a half per cent were able to read a simple sentence from an ordinary school book with intelligence and accuracy. What hopes can be entertained of the improvement of the remaining ninety-five and a half per cent? Education is regarded by statesmen and philanthropists as the lever by which the people are to be elevated, but *education, up to the point of reading and writing to any useful purpose, under present circumstances, is not attained by the great bulk of the population.*" Mr. J. S. Mill remarks: "It is truly a frightful consideration that the annual number of pupils who pass the highest grade in the schools aided by Government, namely, who leave the schools able to read a newspaper with understanding, is less than the number of teachers, including pupil teachers, employed in the schools! There is no doubt that a simplification of English orthography would facilitate considerably the task of learning to read."

If we advance to a higher social grade, the same evil influence manifests itself. Out of 1972 failures in the Civil Service examinations, 1866 candidates were in spelling; that is, eighteen out of every nineteen who failed failed in spelling. Dr. Morell, who states this fact, continues, "It is certain that the ear is no guide in the spelling of English, rather the reverse; and that it is almost necessary to form a personal acquaintance with each individual word."

As another example of reading made hard, let us take an American instance:—From the "Twenty-second Annual

Report of the Board of Trustees of the Public Schools of the City of Washington," we learn that in June, 1866, a spelling match was held, at which there were seven pupils selected by the teachers as the best spellers from each of the eleven intermediate schools. A gold medal had been offered by one of the trustees as a prize for the best speller. The words given out are as follows, with the number of scholars, out of a total of 77, who spelled them incorrectly :—

tambourine	31	indispensable	40	bilious	46	pamphlet	3
complacent	24	susceptible	14	niche	36	labyrinth	42
millinery	21	vignette	44	cedilla	28	ferrule	13
varioloid	52	inveigh	6	horologe	47	facile	46
caterpillar	25	pleurisy	20	exorbitant	31	medicine	4
physiology	16	gauge	20	ellipse	20	flageolet	28
lettuce	2	pallet	17	hierarchy	20	zephyr	9
aloes	16	palate	17	periphery	50	rigid	21
villain	27	palette	48	militia	16	lacquer	23
omelet	27	scurrilous	51	dahlia	30	victuals	8
billiards	5	aeronaut	49	separate	14	surcingle	35
ghoul	39	paroxysm	32	miniature	29	punctilious	33
irresistible	31	daguerreotype	34				

These words were taken from the spelling-books used in the schools. The following are amusing illustrations of the modes of spelling some of the words :—

vereloid	errenaut	skurrelous
variloid	erenote	squerulous
veryaloid	airanaut	scurulous
veraloid	eranoch	scournless
valeloid	arenaught	scirilous
veri O Lord	erenolt	scuroleus
fariloid	erroenort	scurrus
variloyd	eronaut	skireles
bareloyd	aregnout	scurels
barierloid	ereunaut	skirrellous
barryaloid	airinought	schourals
marioloyd	earonaut	scurolous
	arenarch	schurrulous
	aranult	
	erynort	
	arinought	
	arroneut	

A second trial was found necessary, when the medal was awarded to Hattie E. Gove, eleven years of age, of the First District.

"These facts," says the Report, "are presented to show the importance of greater attention being given to this branch of education, so that such a report may never again be presented." What a satire is this on our system of spelling!

The state of the case has been well put in the double statement that no Englishman can tell with certainty how to pronounce any word which is presented to him in the ordinary orthography, unless he has heard it uttered by

others, and no Englishman can tell with certainty how to spell a word with which he is not already familiar in its printed form. In both cases he may *guess*, and his guess will sometimes be right and sometimes be wrong; but in neither case can he attain certainty. The anomalies of English are so great and manifold that it is difficult to exhibit them in a brief compass.

The object of all alphabetic writing is the representation of spoken sounds. For this purpose it is essential that we should have a symbol for each sound, and that that symbol should be used with regularity and consistency. An analysis of the spoken sounds of our language shows that we have thirty-eight distinct sounds, and for the representation of these we have 26 letters, 3 of them mere duplicates. This has led to the device of using two or more letters to indicate a single sound. Had this been done with uniformity all would have been well, but unfortunately no system has been followed. Thus, an examination of 3000 monosyllables showed 145 different methods of indicating the twelve vowel sounds existing in the language. Again, every letter in the alphabet except *j* is mute in some words. Why should we take the useless trouble of writing *b* in the word lamb, seeing that the sound *b* is never heard in it? If we take the entire range of English vowels, we shall find that there are 110 combinations of our imperfect symbols, and that they have 353 meanings. Taking the consonants in the same way, there are 119 combinations having 251 meanings. There are forty ways of representing the vowel sound heard in the word *eel;* there are nineteen ways of writing *k*, *s*, and *n;* there are 26 ways of expressing the vowel heard in *isle*, and there are 37 expedients for showing the vowel in *it*.* Of the many sounds which are hidden under the same symbol, most of us have had ample experience. The difficulties of knowing what sounds to attach to the symbols are equally great

> As a Frenchman once found when he tried to explain
> His complaint, for the spelling so bothered his brain
> That he said to the doctor, "I've got a bad cow;"
> When the doctor could only reply by a bow.
> Again he attempted, "I've got a bad coo;"
> But the doctor was dumb. Seeing that would not do,
> He bethought him again, "I've got a bad co;"
> And he thought that the doctor was terribly slow.

* For a thorough exposition of the thousandfold anomalies of English orthography, we may refer to "A Plea for Phonetic Spelling," by ALEXANDER JOHN ELLIS, B.A. 2nd. ed.; 1848. In this work, all the objections which have been raised against a reformed system of spelling are not only anticipated, but refuted with a clearness and fulness which leaves nothing to be desired.

> But he tried it once more, "I've got a bad cuff;"
> The doctor lost patience and said in a huff,
> "If thus you go on I must take myself off."
> "That's it," cried the Frenchman, "I have got a bad cough."

Now it must be recollected that each of these methods of pronouncing the word cough is sanctioned by the usage in other cases. The analogies of English spelling will justify any absurdity. Take this sentence as an example: "Igh bat ai nyou kought frachm mhy taighlor too-deig."*

Igh	=	I,	as in	n*igh*,
bat	=	bought	,,	f*a*ll,
ai	=	a	,,	pl*ai*d,
nyou	=	new	,,	*you*,
kought	=	coat	,,	th*ough*,
frachm	=	from	,,	yacht,
mhy	=	my	,,	r*hy*me,
taighlor	=	tailor	,,	str*aigh*t,
to	=	to	,,	f*oo*t,
deig	=	day	,,	r*ei*gn.

Let common sense decide. If it is reasonable to represent the vowel *I* by the combination *Igh* in one case, it is reasonable to do so in all. It would only weary to repeat instances of the absurdities of our spelling. They meet us at every turn. If candidates for employment in the Civil Service, who have in most cases been carefully prepared for the ordeal, fail to learn how to spell, what must be the condition of those to whom hard fortune has denied the chance of any large amount of education? How many in the working-classes there must be to whom reading in place of being a solace for the hours of relaxation, and a pleasant method of acquiring knowledge and wisdom, is a thing avoided from the difficulties which beset it.†

This hindrance to the cause of national education, and to the progress of our language abroad having been dwelt upon at some length, it only remains to point out the remedy. Various schemes have from time to time been put forth, but the only one to which attention need now be directed is that advocated by Mr. Isaac Pitman, of Bath. Mr. Pitman, as all the world knows, is the inventor of a very beautiful

* Phonetic Journal, xxx., 426.

† Every one must at times have received letters which show how hopeless is the task of the teachers of elementary schools. I transcribe the body of a letter from a young woman, relating to a fellow-servant:—I ham very soory I connot come to see yu as I ham soo Bessey But I can say that the gril his cleane honest and industers as A tow year good charactar from her last place she his 202 years of age [a startling assertion! The meaning of course is 20+2=22] good temperd and stady.

system of shorthand. Unlike all other stenographies, Phonography is based upon a philosophical analysis of the sounds in the English language, and this analysis has been made the basis of a new Phonetic English Alphabet, in which each sound is indicated by one letter, and each letter is attached to one sound only. In the construction of this alphabet, Mr. Isaac Pitman and Mr. A. J. Ellis, F.R.S.—a man of profound scholarship—were joint-workers.

Since 1843 various modifications have been made in this alphabet, sometimes of doubtful expediency, but always with the single object in view of making it as perfect as possible.

The sanguine expectations of its promoters have not been realised. Mr. Ellis, indeed, appears to have given up all hope of seeing a practical phonetic alphabet adopted, and now proposes a system of digraphs in place of fresh letters, for use concurrently with the old system. This system is much more complicated and cumbersome than the Phonetic alphabet, though infinitely preferable to the present want of system.

Mr. Ellis's Glossic is a new concurrent system of spelling, intended to remedy the defects, without interfering with the use, of existing English orthography.

Key to English Glossic.

Read the large capital letters always in the senses they have in the following words, which are all in the usual spelling except the three underlined, meant for *foot, then, rouge.*

bEEt	bAIt	bAA	cAUl	cOAl	cOOl		
knIt	nEt	gnAt	nOt	nUt	fUOt		
hEIght	fOIl	fOUl	fEUd				
Yea	Way	WHey	Hay				
Pea	Bee	Toe	Doe	CHest	Jest	Keep	Gape
Fie	Vie	THin	DHen	Seal	Zeal	ruSH	rouZHe
eaR	R'ing	eaRR'ing	Lay	May	Nay	siNG	

R is vocal when no vowel follows, and modifies the preceding vowel, forming diphthongs, as in pEER, pAIR, bOAR, bOOR, hERb.

Use R for R', and RR for RR', when a vowel follows, except in elementary books, where *r'* is retained.

Separate *th, dh, sh, zh, ng* by a hyphen (-) when necessary.

Read a stress on the first syllable when not otherwise directed.

Mark stress by (·) after a long vowel or *ei, oi, ou, eu,* and after the first consonant following a short vowel.

Mark emphasis by (·) before a word.

Pronounce *el, em, en, er, ej, a,* obscurely, after the stress syllable.

When three or more letters come together of which the two *first* may form a digraph, read them as such.

Letters retain their usual names, and alphabetical arrangement.

Words in customary or NOMIC spelling occurring among GLOSSIC, and conversely, should be underlined with a wavy line ~~~~, and printed with s p a i s t l e t e r z, or else in a *different type.*

Spesimen ov Ingglish Glosik.

OBJEKTS.

Too fasil·itait Lerning too Reed.

Too maik Lerning too Spell unnes·eseri.

Too asim·ilait Reeding and Reiting too Heerring and Speeking.

Too maik dhi Risee·vd Proanunsiai·shen ov Ingglish akses·ibl too *aul* Reederz, Proavin·shel and Foren.

MEENZ.

Leev dhi Ould Speling untuch·t.

Introadeu·s along· seid ov dhi Oald Speling a Neu Aurthog·rafi, konsis·ting ov dhi Oald Leterz euzd in-vai·rriabli in dhair best noan sensez.

Emploi· dhi New Speling in Skoolz too Teech Reeding in *boath* Aurthog·rafiz.

Alou· eni Reiter too reit in dhi Neu Speling *oanli* on aul okai·zhenz, withhou·t loozing kaast, proavei·ded hee euzez a Risee·vd Proanunsiai·shen; dhat is—

Aknol·ej dhi Neu Speling konkur·entli widh dhi Oald.

Mr. Melville Bell's is probably the most philosophical system yet invented for the representation of vocal sounds, but its chances of adoption as the vehicle of English are too remote to need more than passing allusion.

Various other schemes, more or less thorough, have been devised for remedying the defects of English orthography, but none of them have attained the same importance as Mr. Pitman's proposals. The immense circulation of his shorthand has had the effect of familiarising the public mind with the theory of phonetic analysis and representation. For a generation he has spread information on the subject, and gathered round him a band of devoted adherents and disciples. His system is now the only system of phonetic English which has any chance of success. There is a yearly-increasing literature printed in it, and it may be hoped that the present national feeling in favour of education will aid its promoters against the present education-hindering system.

It may appear a sweeping change to alter the form and aspect of the language, but the change is by no means so violent as it seems. Changes in spelling are constantly taking place, but they are alterations which come about by hazard and without system.

If other nations have succeeded in reforming their orthography, and we know this to be the case with the Dutch

THE PHONETIC ALPHABET.

The Phonetic Alphabet consists of 38 letters, namely, the 23 useful letters of the common alphabet (*c*, *q*, and *x* being rejected), and the 15 new ones below. The vowels *a*, *e*, *i*, *o*, *u* have invariably their short sounds, as in *pat*, *pet*, *pit*, *pot*, *put*. All the other old letters have their usual signification. The italic letters in the words in the fourth line denote the SOUNDS of the letters.

VOWELS.								DIPHTHONGS.		CONSONANTS.					
Ꞗ ɞ,	Ɛ ɛ,	Ɩ ı:	Ꞷ ꞷ,	Ꞙ ɷ,	Ɯ ɯ:	Ɤ ɤ.		Ɨ ɨ,	Ʉ ʉ.	Ꞔ ꞔ,	Ꞓ ꞓ,	Ꟈ ꟈ,	Σ ʃ,	Ʒ ʒ,	Ŋ ŋ.
Ꞗ ɞ,	*Ɛ ɛ,*	*Ɩ ı:*	*Ꞷ ꞷ,*	*Ꞙ ɷ,*	*Ɯ ɯ:*	*Ɤ ɤ.*		*Ɨ ɨ,*	*Ʉ ʉ.*	*Ꞔ ꞔ,*	*Ꞓ θ,*	*Ꟈ ꟈ,*	*Σ ʃ,*	*Ʒ ʒ,*	*Ŋ ŋ.*
Ꞗ ɞ	*Ɛ ɛ*	*Ɩ ı*	*Ꞷ ꞷ*	*Ꞙ ɷ*	*Ɯ ɯ*	*Ɤ ɤ*		*Ɨ ɨ*	*Ʉ ʉ*	*Ꞔ ꞔ*	*Ꞓ θ*	*Ꟈ ð*	*Σ ʃ*	*Ʒ ʒ*	*Ŋ ŋ*
alms,	*a*ge, *a*ir,	*ea*t:	*a*ll,	*o*pe,	f*oo*d:	*s*on, b*u*t.		b*y*,	n*ew*.	*ch*air,	*th*in,	*th*en,	*sh*oe,	vi*s*ion,	si*ng*.
ɞmz,	ɛɨ, ɛr,	ıt:	ꞷl,	ɷp,	fɯd:	sɤn, bɤt.		bɨ,	nʉ.	ꞔɛr,	ꞓin,	ꟈen,	ʃɯ,	viʒon,	siŋ.

The order of the Phonetic Alphabet, and the names of the letters are—

CONSONANTS:—p, b; t, d; ꞔ, j; k, g: f, v; ꞓ, ꟈ; s, z; ʃ, ʒ: m, n, ŋ: l, r: w, y: h.
pee, bee; tee, dee; chay, jay; kay, gay: ef, vee; ith, thee; es, zee; ish, zhee: em, en, ing: el, ar: way, yay: aitch.

VOWELS:—a, ɞ; e, ɛ; i, ı: o, ꞷ; ɤ, ɷ; u, ɯ.
at, ah; et, eh; it, ee: ot, awe; ut, oh; ɤɤt, oo.

DIPHTHONGS:—ɨ, ʉ, ou, oi.
eye, ew, ow, oy.

FOREIGN SOUNDS:—FRENCH Œ œ, Ʊ ʊ *Ʊ ʊ*, Ɯ ɯ *Ɯ ɯ*, Ƞ ƞ *Ƞ ƞ*; GER. X x, Ɥ ɥ.
as in *jeûne*, *du*, *dû*, *an*; *ich*, *Sieg.*

20 And h.i lifted ɜp hiz įz on hiz disįpelz, and sed, Blesed b.i y.i pɯr
for ųr'z iz đe kiŋdom ov God.
21 Blesed ar y.i đat hɜŋger nou: for y.i ʃal b.i fild. Blesed ar y.i
đat w.ip nou: for y.i ʃal lɛf.
22 Blesed ar y.i, when men ʃal hɛt ų, and when đɛ ʃal separɛt ų
from đɛr kɜmpani, and ʃal reprɵç ų, and kast out ųr nɛm az .ivil, for
đe Sɜn ov man'z sɛk.
23 Rejois y.i in đát dɛ, and l.ip for joi: for, behɵld, ųr reword iz
grɛt in heven: for in đe lįk maner did đɛr fɛđerz ɜntu đe profets.
24 Bɜt wɵ ɜntu ų đat ar riç! for y.i hav res.ivd ųr konsɵlɛʃon.
25 Wɵ ɜntu ų đat ar ful! for y.i ʃal hɜŋger. Wɵ ɜntu ų đat lɛf
nou! for y.i ʃal mɵrn and w.ip.
26 Wɵ ɜntu ų, when ɷl men ʃal sp.ik wel ov ų! for sɵ did đɛr fɛ-
đerz tu đe fɷls profets.
27 Bɜt į sɛ ɜntu ų whiç h.ir, Lɜv ųr enemiz, dɯ gud tu đem whiç
hɛt ų,
28 Bles đem đat kɜrs ų, and prɛ for đem whiç despįtfuli ųz ų.
29 And ɜntu him đat smįteŧ đ.i on đe wɜn çik ofer ɷlsɵ đe ɜđer;
and him đat tɛkeŧ awɛ đį klɵk forbid not tu tɛk đį kɵt ɷlsɵ.
30 Giv tu everi man đat askeŧ ov đ.i; and ov him đat tɛkeŧ awɛ
đį gudz ask đem not agen.
31 And az y.i wud đat men ʃud dɯ tu ų, dɯ y.i ɷlsɵ tu đem lįkwįz.
32 For if y.i lɜv đem whiç lɜv ų, whot ŧaŋk hav y.i? for sinerz ɷlsɵ
lɜv đɵz đat lɜv đem.
33 And if y.i dɯ gud tu đem whiç dɯ gud tu ų, whot ŧaŋk hav y.i?
for sinerz ɷlsɵ dɯ .iven đe sɛm.
34 And if y.i lend tu đem ov hɯm y.i hɵp tu res.iv, whot ŧaŋk hav
y.i? for sinerz ɷlsɵ lend tu sinerz, tu res.iv az mɜç agen.
35 Bɜt lɜv y.i ųr enemiz, and dɯ gud, and lend, hɵpiŋ for nɜŧiŋ
agen; and ųr reword ʃal b.i grɛt, and y.i ʃal b.i đe çildren ov đe Hįest:
for h.i iz kįnd ɜntu đe ɜnŧaŋkful and tu đe .ivil.
36 B.i y.i đɛrfɵr mersiful, az ųr fɛđer ɷlsɵ iz mersiful.
37 Jɜj not, and y.i ʃal not b.i jɜjd: kondem not, and y.i ʃal not b.i
kondemd: forgiv, and y.i ʃal b.i forgiven:
38 Giv, and it ʃal b.i given ɜntu ų; gud mezur, prest doun, and
ʃɛken tugeđer, and rɜniŋ ɵver, ʃal men giv intu ųr bɯzom. For wiđ
đe sɛm mezur đat y.i m.it wiđɷl it ʃal b.i mezurd tu ų agen.
39 And h.i spɛk a parabel ɜntu đem, Kan đe blįnd l.id đe blįnd? ʃal
đɛ not bɵŧ fɷl intu đe diç?
40 Đe disįpel iz not abɜv hiz master: bɜt everiwɜn đat iz perfekt
ʃal b.i az hiz master.
41 And whį behɵldest đou đe mɵt đat iz in đį brɜđer'z į, bɜt
pers.ivest not đe b.im đat iz in đįn ɵn į?
42 Ŧder hou kanst đou sɛ tu đį brɜđer, Brɜđer, let m.i pul out đe
mɵt đat iz in đįn į, when đou đįself behɵldest not đe b.im đat iz in
đįn ɵn į? Đou hipokrit, kast out ferst đe b.im out ov đįn ɵn į, and
đen ʃalt đou s.i kl.irli tu pul out đe mɵt đat iz in đį brɜđer'z į.
43 For a gud tr.i briŋeŧ not fɵrŧ korɜpt frɯt; nįđer dɜŧ a korɜpt
tr.i briŋ fɵrŧ gud frɯt.
44 For everi tr.i iz nɵn bį hiz ɵn frɯt. For ov ŧornz men dɯ not
gađer figz, nor ov a brambel buʃ gađer đe grɛps.
45 A gud man out ov đe gud trezur ov hiz hart briŋeŧ fɵrŧ đát
whiç iz gud; and an .ivil man out ov đe .ivil trezur ov hiz hart briŋeŧ
fɵrŧ đát whiç iz .ivil; for ov đe abɜndans ov đe hart hiz mouŧ sp.ikeŧ.

A VIEW OF THE PHONETIC ALPHABET,

IN VARIOUS STYLES OF WRITING AND PRINTING.

CONSONANTS.

Examples	Roman.	Old English.	Italic.	Script.	Short-hand.	Name.
peep	P p	P p	*P p*	P p		pee
bib	B b	B b	*B b*	B b		bee
tight	T t	T t	*T t*	T t		tee
deed	D d	D d	*D d*	D d		dee
church	Є ç	Є ç	*Є ç*	Є ç		chay
judge	J j	J j	*J j*	J j		jay
cake	K k	K k	*K k*	K k		kay
gig	G g	G g	*G g*	G g		gay
faith	F f	F f	*F f*	F f		ef
valve	V v	V v	*V v*	V v		vee
bath	Ŧ ŧ	Ŧ ŧ	*Ŧ ŧ*	Ŧ ŧ		ith
bathe	Ƌ đ	Ƌ đ	*Ƌ đ*	Ƌ đ		thee
sauce	S s	S s	*S s*	S s		ess
size	Z z	Z z	*Z z*	Z z		zee
ship	Σ ʃ	Σ ʃ	*Σ ʃ*	Σ ʃ		ish
azure	Ʒ ʒ	Ʒ ʒ	*Ʒ ʒ*	Ʒ ʒ		zhee
maim	M m	M m	*M m*	M m		em
noon	N n	N n	*N n*	N n		en
sing	Ŋ ŋ	Ŋ ŋ	*Ŋ ŋ*	Ŋ ŋ		ing
lull	L l	L l	*L l*	L l		el
roar	R r	R r	*R r*	R r		ar
way	W w	W w	*W w*	W w		way
yea	Y y	Y y	*Y y*	Y y		yea
hay	H h	H h	*H h*	H h		aitch

VOWELS.

Examples	Roman.	Old English.	Italic.	Script.	Short-hand.	Name.
pat	A a	A a	*A a*	A a		at
alms	Ɑ ɑ	Ɑ ɑ	*Ɑ ɑ*	Ɑ ɑ		ah
pet	E e	E e	*E e*	E e		et
age	Ɛ ɛ	Ɛ ɛ	*Ɛ ɛ*	Ɛ ɛ		eh
pit	I i	I i	*I i*	I i		it
eat	Ɩ ɩ	Ɩ ɩ	*Ɩ ɩ*	Ɩ ɩ		ee
pot	O o	O o	*O o*	O o		ot
all	Ꞷ ɷ	Ꞷ ɷ	*Ꞷ ɷ*	Ꞷ ɷ		aw
but	Ɣ ɤ	Ɣ ɤ	*Ɣ ɤ*	Ɣ ɤ		ut
old	Ɵ ɵ	Ɵ ɵ	*Ɵ ɵ*	Ɵ ɵ		oh
put	U u	U u	*U u*	U u		ŏŏt
ooze	Ɯ ɯ	Ɯ ɯ	*Ɯ ɯ*	Ɯ ɯ		ōō

DIPHTHONGS.

Examples	Roman.	Old English.	Italic.	Script.	Short-hand.	Name.
my	Ɨ ɨ	Ɨ ɨ	*Ɨ ɨ*	Ɨ ɨ		eye
new	Ʉ ɥ	Ʉ ɥ	*Ʉ ɥ*	Ʉ ɥ		you

The diphthongs in "ay (yes), boy, boil, now, noun," are written by the single letters that represent their elements, thus:

ai   oi   ou

The Phonetic Alphabet consists of 38 letters, namely, the 23 useful letters of the common alphabet (*c*, *q*, and *x* being rejected,) and 15 new ones. The vowels *a*, *e*, *i*, *o*, *u* have invariably their short sounds, as in *put*, *pet*, *pit*, *pot*, *put*. All the other old letters have their usual signification.

SPECIMEN OF PHONETIC PRINTING.

Bɨ đe Fɵnetik Alfabet eni person, ɵld or yɤŋ, mɛ bi tɵt tu rɩd, bɵŧ in fɵnetik and in ordinari buks, in ŧri mɤnŧs,—ai, ofen in twenti ourz' instrɤkʃon,—a task whiç iz rɛrli akomplıʃt in ŧri yɩrz ov toil bɨ đe ɵld alfabet. Whot fɑđer or tiçer wil not hɛl đis grɛt bun tu edɥkɛʃon?—đis pouerful maʃin for đe difɥʒon ov nolej!

Luke 6. 20-45.

20 And hɩ lifted ɤp hiz ɨz on hiz disɨpelz, and sed, Blesed bɩ yɩ pɯr
for ɥr'z iz đe kiŋdom ov God.
21 Blesed ar yɩ đat hɤŋger nou: for yɩ ʃal bɩ fild. Blesed ar yɩ
đat wɩp nou: for yɩ ʃal lɑf.
22 Blesed ar yɩ, when men ʃal hɛt ɥ, and when đɛ ʃal separɛt ɥ
from đer kɤmpani, and ʃal reprɵç ɥ, and kast out ɥr nɛm az ɩvil, for
đe Sɤn ov man'z sɛk.
23 Rejois yɩ in đát dɛ, and lɩp for joi: for, behɵld, ɥr reword iz
grɛt in heven: for in đe lɨk maner did đer fɑđerz ɤntu đe profets.
24 Bɤt wɵ ɤntu ɥ đat ar riç! for yɩ hav resɩvd ɥr konsɵlɛʃon.
25 Wɵ ɤntu ɥ đat ar ful! for yɩ ʃal hɤŋger. Wɵ ɤntu ɥ đat lɑf
nou! for yɩ ʃal mɵrn and wɩp.
26 Wɵ ɤntu ɥ, when ɷl men ʃal spɩk wel ov ɥ! for sɵ did đer fɑ-
đerz tu đe fɷls profets.
27 Bɤt ɨ sɛ ɤntu ɥ whiç hɩr, Lɤv ɥr enemiz, dɯ gud tu đem whiç
hɛt ɥ,
28 Bles đem đat kɤrs ɥ, and prɛ for đem whiç despɨtfuli ɥz ɥ.
29 And ɤntu him đat smɨteŧ đɩ on đe wɤn çɩk ofer ɷlsɵ đe ɤđer;
and him đat tɛkeŧ awɛ đɨ klɵk forbid not tu tɛk đɨ kɵt ɷlsɵ.
30 Giv tu everi man đat askeŧ ov đɩ; and ov him đat tɛkeŧ awɛ
đɨ gudz ask đem not agen.
31 And az yɩ wud đat men ʃud dɯ tu ɥ, dɯ yɩ ɷlsɵ tu đem lɨkwɨz.
32 For if yɩ lɤv đem whiç lɤv ɥ, whot ŧaŋk hav yɩ? for sinerz ɷlsɵ
lɤv đɵz đat lɤv đem.
33 And if yɩ dɯ gud tu đem whiç dɯ gud tu ɥ, whot ŧaŋk hav yɩ?
for sinerz ɷlsɵ dɯ ɩven đe sɛm.
34 And if yɩ lend tu đem ov hɯm yɩ hɵp tu resɩv, whot ŧaŋk hav
yɩ? for sinerz ɷlsɵ lend tu sinerz, tu resɩv az mɤç agen.
35 Bɤt lɤv yɩ ɥr enemiz, and dɯ gud, and lend, hɵpiŋ for nɤŧiŋ
agen; and ɥr reword ʃal bɩ grɛt, and yɩ ʃal bɩ đe çildren ov đe Hɨest:
for hɩ iz kɨnd ɤntu đe ɤnŧaŋkful and tu đe ɩvil.
36 Bɩ yɩ đerfɵr mersiful, az ɥr fɑđer ɷlsɵ iz mersiful.
37 Jɤj not, and yɩ ʃal not bɩ jɤjd: kondem not, and yɩ ʃal not bɩ
kondemd: forgiv, and yɩ ʃal bɩ forgiven:
38 Giv, and it ʃal bɩ given ɤntu ɥ; gud mezur, prest doun, and
ʃɛken tugeđer, and rɤniŋ ɵver, ʃal men giv intu ɥr bɯzom. For wiđ
đe sɛm mezur đat yɩ mɩt widɷl it ʃal bɩ mezurd tu ɥ agen.
39 And hɩ spɛk a parabel ɤntu đem, Kan đe blɨnd lɩd đe blɨnd? ʃal
đɛ not bɵŧ fɷl intu đe diç?
40 Ƌe disɨpel iz not abɤv hiz master: bɤt everiwɤn đat iz perfekt
ʃal bɩ az hiz master.
41 And whɨ behɵldest đou đe mɵt đat iz in đɨ brɤđer'z ɨ, bɤt
persɩvest not đe bɩm đat iz in đɨn ɵn ɨ?
42 Ɨđer hou kanst đou sɛ tu đɨ brɤđer, Brɤđer, let mɩ pul out đe
mɵt đat iz in đɨn ɨ, when đou đɨself behɵldest not đe bɩm đat iz in
đɨn ɵn ɨ? Ƌou hipokrit, kast out ferst đe bɩm out ov đɨn ɵn ɨ, and
đen ʃalt đou sɩ klɩrli tu pul out đe mɵt đat iz in đɨ brɤđer'z ɨ.
43 For a gud trɩ briŋeŧ not fɵrŧ korɤpt frɯt; nɨđer dɵŧ a korɤpt
trɩ briŋ fɵrŧ gud frɯt.
44 For everi trɩ iz nɵn bɨ hiz ɵn frɯt. For ov ŧornz men dɯ not
gađer figz, nor ov a brambel buʃ gađer đe grɛps.
45 A gud man out ov đe gud trezur ov hiz hart briŋeŧ fɵrŧ đát
whiç iz gud; and an ɩvil man out ov đe ɩvil trezur ov hiz hart briŋeŧ
fɵrŧ đát whiç iz ɩvil; for ov đe abɤndans ov đe hart hiz mouŧ spɩkeŧ.

and the Spanish, surely we may hope for success also in the same undertaking.* And when that day comes on which we have swept away what Max Müller has well called "our corrupt and effete orthography," we shall have destroyed the last and only barrier which prevents English from being the language of the world.†

Surely that is a future so great and glorious that we need not hesitate at any trouble which will hasten the day. We have already achieved much. The flowers that first grew beside the Avon, now bloom alike on the banks of the sacred Ganges, and by the margin of the broad Mississippi. The lays of merry England are heard alike in the fair Derbyshire dales and on the plains of the Far West. The thoughts of our great thinkers, the songs of our poets are no longer bounded by the narrow seas that hem in our island home. They fly to every point of the compass, and

* One of the first undertakings of the Real Academia Espanola was to reform the Spanish spelling, to make it uniform in principle and easy in practice. The first of the rules laid down was, that "the pronunciation of a word should be the sole and universal rule for its orthography, when it is sufficient to determine the various letters." The result is that "the orthography of Spanish at the present day leaves little for the phonetician to desire, as it suffices to determine the pronunciation of every word with ease and certainty." Dutch spelling was re-modelled by Professor Siegenbeek, and since 1806 it has been required by the Government that all public documents should be written by his system. Polish, Bohemian, and Magyar have modern alphabets, and are constructed on strictly phonetic principles.—ELLIS, "Plea," pp, 59, 60.

† The literature of spelling-reform is already extensive. The following represent the most important proposals:—

"A Plea for Phonetic Spelling; or, the Necessity of Orthographic Reform." By ALEXANDER JOHN ELLIS, B.A. Second edition. London, 1848. 8vo.

"The Essentials of Phonetics, containing the theory of a universal alphabet, together with its practical application as an ethnical alphabet to the reduction of all languages written or unwritten, to one uniform system of writing." By ALEXANDER JOHN ELLIS, B.A. London, 1848. 8vo. This is printed in the "Phonetic Alphabet" of 1847.

"On Early English Pronunciation, with especial reference to Shakspere and Chaucer, containing an investigation of the correspondence of writing with speech in England from the Anglo-Saxon period to the present day, preceded by a systematic notation of all spoken sounds by means of the ordinary printing types." By ALEXANDER J. ELLIS, F.R.S., F.S.A. 1869-71. Parts 1 to 3.

"A Defence of Phonetic Spelling, drawn from a history of the English alphabet and orthography, with a remedy for their defects." By R. G. LATHAM, M.A., M.D., F.R.S. Bath, 1872. 8vo.

"The Universal Language," an argument for the reformed orthography, as a means of aiding the universal diffusion of the English language. By WILLIAM WHITE, Bath. 12mo., pp. 16.

Mr. Isaac Pitman has for thirty years printed a Phonetic Journal, which has now become a repository of nearly everything of importance that has been issued on the subject. He has also issued numerous tracts in advocacy of his proposals.

"Visible Speech the Science of Universal Alphabetics," or self-interpreting physiological alphabetics for the writing of all languages in one alphabet. By ALEXANDER MELVILLE BELL. London, 1867.

find everywhere audiences not few but fit. In the Australian sheep-walk, amid the tropical glories of Jamaican scenery, in the glowing valleys of the Polynesian islands, east, west, north, or south, we find the restless energetic Englishman. It is not a thing to be lightly thought of, this wide extension of our English tongue.

Our language is a beautiful casket, shining with gold and glittering with gems, and enclosing still more precious, still more costly jewels. Wherever the Englishman goes he carries with him the energy, the love of order, the purity of home-life, the independence, the freedom of thought, of speech, of action, which have made England not only great and prosperous, but the "august mother of free nations." The language is the best test of national capacity. It expresses not only the exact extent of the nation's knowledge, but also its spiritual condition and moral aspirations. Apart from all national vanity, we may rejoice that Shakspere's language is going forth to the ends of the earth. It bears with it the science of Newton and the politics of Adam Smith. It bears with all that is purest and best in the teachings of the ancient world. It bears with it countless memories of heroic deeds. It bears with it those aspirations after Liberty and Right which are the most precious possession of our race. May it go forward conquering and to conquer, resistless in its power and majesty, until it becomes a new bond of peace and brotherhood amongst all the nations, until earth's fertile valleys shall glow with fruits and flowers, and "the desert shall rejoice and blossom as the rose."

IX. THE SCIENTIFIC ASPECT OF THE INTERNATIONAL EXHIBITION, 1873.

THE exhibitions at South Kensington, in their annual occurrence, are losing much of their novelty, and are assuming that business character which must be essential to their wholesome effect upon the national industry. An Englishman in all he does is always very much in earnest, and our exhibitions have been characterised throughout by a determination not "to play at work." In "going to the Exhibition" there is something indicative of real work, very different to the idea that obtained with the "World's Show" of 1851. As a record of progress,

scientific, artistic, or commercial, our exhibitions have thrived best, and not as a place of recreation. So that one is not astonished at the serious, business-like aspect of the now familiar galleries, which present amongst their contents perhaps not so much novelty, but certainly quite as much interest as the exhibits of former years.

It is perhaps premature to speak of the effects of these institutions upon the industrial classes, but doubtless many of our readers will have remarked,—and nowhere is it more apparent than at the exhibition itself,—the increased interest and desire for information. It should be remembered that the number of visitors attending the exhibition of the present year represents very nearly the proportion who attend for the purposes, not of recreation, but to satisfy the desire to acquire knowledge. The novelty has long ago worn off. The largeness of the attendance is most encouraging, and evinces a permanent and wide-established wish to maintain our national commercial standard.

Another and not less important feature is the increased value of our colonial exhibits, regarded from an artistic and adaptable point of view. These exhibits have been following a steady course of development, especially in independence of character. And if we look upon our colonial possessions as outposts in the English army of civilisation, we shall derive much profitable pleasure from the contemplation of their improvement.

Considering the attractions of the Vienna Exhibition, the portion of our exhibition absorbed by continental exhibitors is most creditable to the industry and perseverance of our authorities, for many very interesting works and processes come from abroad.

The portion of the exhibition possessing the greatest interest to the general scientific visitor is the machinery departments and their adjuncts. And here there is this year an exhibit of the highest order of merit,—the rearing of the silkworm, and the processes of preparing, spinning, and weaving silken fibre. Immediately outside the machinery department is a quiet, neat tent, containing, on trays supported by a framework in the centre of the structure, many thousand silkworms. Here the development of this wonderful and valuable insect is witnessed upon a commercial scale, as exhibited by M. Alfred Roland, of Orbe, Switzerland. Returning to the machinery department, we stand in front of M. Jouffray's (Rue Vimaine, Vienna) apparatus for unwinding the cocoon of the silkworm. The establishments in which the unwinding is carried on are

termed "filatures;" and the machinery consists of ordinary reels driven sometimes by a falling weight, or machinery of a very crude order. Here, of course, steam is employed. A table with a brass top contains shallow tinned-copper boilers, about a foot in diameter, and nine inches in depth. One of these boilers is heated by steam, and on the surface of the water there float several score of cocoons. A whisk of peculiar shape is immersed in the water by the operator, and rapidly rotated; when withdrawn from the water there are attached to the whisk several of the ends of the cocoons, and these fibres are passed to the reel. A most important part of the process consists in maintaining a constant supply of fibre to this compound thread from the unattached cocoons. The compound fibre passes over a circular glass hook to a horizontal bobbin, upon which it is wound, motion being imparted to the bobbin by a small wooden roller on the bobbin-spindle. This "winding" machine, as well as the "cleaning," "doubling," and "spinning" machines, are exhibited by Messrs. Rushton, Sons, and Co., of Macclesfield. The "cleaning" machine next takes up the silk, and transfers it to another bobbin. During its passage, the silk passes between two fixed parallel plates close together. By this means any irregularity or knot in the fibre is detected. In the doubling machine the fibres from two or three bobbins are wound side by side, without twisting, on to one bobbin. A very neat contrivance is employed to detect a break in any one of the fibres, and so to prevent inequality in the thickness of the silk. The breaking it would be tiresome to detect by the eye, because the filaments are so fine as to be difficultly visible. The filament is passed through an eye in the end of a wire, and supports this wire. Should the filament break, the wire falls, and liberates a friction cam, which, pressing against the bobbin, stops it. On the spinning machine the compounded fibre from the doubling machine is twisted, and this spinning is completed by a fifth machine, whence the silk proceeding is commercially known as "tram" and "organzine," according to the mode of its spinning. "Tram" is the term applied to the fibres with a minimum of twist, and "organzine" to those with a maximum twist. The twists are about twenty to the inch of thread. The silk is now handed over to the dyer, who, in turn, when his processes are complete, forwards it to the weaver. But before the dyer takes the silk in hand, a piece of mechanism known as the "snail cam" is employed to arrange the silk in hanks. A complete set of the interesting apparatus used

in the preparation of the fibre is also exhibited by Messrs. W. Higginbottom, of Derby. Messrs. Greenwood and Batley, of Leeds, exhibit a machine for the utilisation of the waste silk of the foregoing processes, which is effected by a similar process to that employed in cotton manufacture, and described before in this journal. Messrs. Warner, Sillett, and Ramm, of Newgate Street, and Messrs. Norris and Co., of Wood Street, Cheapside, exhibit three Jacquard looms, worked by manual labour, and the design from which the cards that control the action of the machine are prepared. The design is by Owen Jones, Esq., and is divided into 5,587,200 small squares, the design to be placed on the cards being selected from these squares. It is impossible, however, to give an exhaustive account of these pieces of superb mechanism; but we may select the following from the excellent report prepared for the Society of Arts by the Rev. Arthur Rigg, M.A. "It will be observed," he says, "that at the top of these three looms there are a number of cards in which holes are perforated. The holes in each card represent some of the squares in the pattern through which the needle of an embroiderer would pass, assuming the design to be one for tapestry. To form the design exhibited, there are connected 9312 cards in three lines. These cards are laced together, and measure 1000 yards in length. The whole pack has to be turned over each time that the design is completed in the loom. Immediately under the one top card in each line of cards there is a square metal boxing, filled on all sides with small holes; in fact, honeycombed, but with square instead of hexagonal cells. These boxings are on axes, in one and the same straight line, and by means of a catch, connected with a cord on which the workman's hand or foot can act, they may be turned through one-fourth of the circumference by one motion of the hand or foot. In so turning, the perforated cards are drawn forward, each card covering one side of the square boxing, except where the holes in the cards previously alluded to are found. For this turning, the frame-work in which these boxes rotate is caused to move on one side. If the cards and the square boxings were taken away, there would be seen a number of wires projecting horizontally from a series of openings in a fixed metal framing. Each of these wires is held forward by means of light coiled springs at the back ends of them. If now the square boxings with perforated cards over the back vertical side be permitted to fall upon the projecting wires, a number of them will be pressed back against the light springs, the

remainder passing through the perforations in the cards and entering the honeycombed box. Between the visible ends and back springs each wire is bent round so as to form an eye through which a vertical wire passes. These vertical wires have hooks at the top and bottom. Cords, hereafter alluded to, are attached to the bottom hooks. The top hooks of those wires, through eyes pressed back by the cards, are thrown out of the general line; and thus, when a narrow metal slip is raised by the hand or foot of the workman, those vertical wires only are raised which remain in the normal line, and therefore those lower hooks only are moved which form part of these wires. A number of cords pass from the warp to these hooks; concealed by the numerous threads of the warp are small delicate little glass frames, each containing six very closely-formed eyes, placed vertically over one another; to the top eye a cord from a hook is attached—through the next four eyes four adjoining threads of the warp pass,—to the lower eye is fastened a cord with a light leaden weight; thus the 29,088 of the warp are passed through these eyes. When, now, the wires are raised to which cords are attached, four times that number of threads are raised. But it may be requisite that only one or two of these four should have been raised. An arrangement for this purpose is made in hanging framings of threads near the operator's hands. These framings constitute what is named "a harness;" in them every thread in the warp has an eye to itself, and therefore, by the action of these eight framings, one or more of the raised threads can be depressed or raised higher. This "harness" is not required where, as in Messrs. Stevens's (of Coventry) loom, each thread has a cord and eye to itself."

Not the least important portion of the Exhibition are the Food Processes, even if we exclude for the present Mr. Buckmaster's School of Cookery, to which we will afterwards refer. Though sweetmeats can scarcely be termed food, yet they may be conveniently classed as an adjunct; and it will be better, as following next in catalogue order, to inspect the machinery and processes employed in the manufacture of sugar confectionery exhibited by Messrs. F. Allen and by Messrs. Hill and Jones. These machines consist essentially of immense copper pans revolving eccentrically, which contain the seeds or almonds to be sugared. Liquid sugar is admitted to the seeds or almonds, and these kept constantly rolling by the motion of the pans are soon covered with a thin coat of sugar. Sugar is again added, until a sufficiently thick coat is obtained. The resulting sugar-

almonds or carraway comfits are coloured, if required, by the introduction of colouring matter into the pan during the final coating. A description is given, although the process is not yet shown (by Messrs. Hill and Jones), of the method by which essences and liqueurs are confined within sugar vessels:—Shallow trays, about fifteen inches wide, are filled with starch flour. A "strike," or levelling edge is drawn over, and the surface thereby smoothed. On the under side of a narrow board, about eighteen inches long and four inches broad, are fastened a number of plaster-of-Paris moulds, of the forms to be made. These narrow boards are laid on the starch flour again and again, until the surface is indented with the designs. A pan of clarified sugar, at such a temperature and consistency as the workman deems suitable, is added to it and well stirred in the non-crystallisable liquid. Each design is thus filled with a crystallisable and non-crystallisable substance, and the manufacturer takes advantage of a physical law, that under these conditions the crystalline element squeezes into the interior the non-crystalline one. Mr. Rigg thinks this method of making confectionery suggestive of the vesicular cavities coming under the notice of the geologist and mineralogist in agates, &c., and he refers to Nicol's paper, "On Fluid in Minerals," given in the "Edinburgh Philosophical Journal," for 1828-9.

Near at hand, Messrs. Tulloch and Co. exhibit some cocoa-flaking machinery, the cocoa being forced between a fixed edge and a rotating disc. Messrs. Tallerman show their process of preserving meat by the immersion of the cases containing the meat in chloride of calcium. Messrs. Criscuolo, Kay, and Co. have a very interesting exhibit illustrating the method of manufacturing maccaroni, in which Semolina wheat is kneaded into a dry dough, the dough being forced through a heated cylinder, and then through apertures of the size the maccaroni is intended to assume, whence it is taken to the drying-room. Another apparatus for preparing cocoa is shown by the Compagnie Francaise. Farther on is the machinery devised by Messrs. Colman for separating pure mustard from the seed, and a most noisy exhibit it is. The seed is first crushed between steel rollers, and then in a stamping mill, whence it is transferred to a series of sieves shaken by mechanical means. At the same time is shown the method of constructing the canisters and the cases in which these are packed. Messrs. Car and Cunningham exhibit a "disintegrating flour mill," and Messrs. Batty the preparation of oranges for marmalade.

The manufacture of aërated waters is always a matter of interest, and the visitor will be well rewarded by a study of the machines and methods exhibited by Messrs. Hayward, Tyler, and Co., by Messrs. Barnet and Foster, and by Messrs. Fleet and Co. The chief difference in these methods is in closing the bottles. By Messrs. Barnet and Foster the bottle is closed with a marble pressed against an india-rubber welt by the force of the gas, while Messrs. Hayward and Co. employ a wooden plug to effect the same purpose.

We may class together the peculiar machinery employed in working or crushing stone. First, in catalogued order, there is an exhibit by the Diamond Rock Boring Company of their drills for mining, quarrying, &c. The black hard carbons are fixed in a collar at the end of a tube, and are made to rotate on the face of the rock to be bored. Messrs. H. R. Marsden exhibit a machine for crushing ores or breaking stones, consisting in the application of corrugated powerful jaws to this purpose. But by far the most unique exhibit is the sand-blast of Messrs. Tilghman, which has already been described in the pages of this journal. It will not, however, be uncalled for to give again the principles of this invention. The force employed is the abrading action of minute particles of sand (impelled by a steam or air-blast) when brought into contact with a hard, resisting surface, as that of stone, glass, &c. By covering the portion of the surface which it is desired should remain uncut with a medium having but slight elasticity, as paper, india-rubber, designs may be produced upon the surface, or cut entirely through by continuing the action. The sand is admitted from a hopper into an inner tube surrounded by a steam-jet, the steam being supplied from 55 lbs. boiler pressure. In five minutes three-sixteenths of an inch of marble were cut away. In this process, by means of chromatised gelatine, photographs may be taken and cut into glass.

We must pass by the silks and velvets, for our time is running short, and there are still the surgical appliances to be seen. These are arranged in the west theatre on the balcony floor of the Royal Albert Hall, and include not only the most modern improvements, but a historical collection extending back to the time of Greek medicine. In this room the instruments, where they are not self-explanatory, need a special medical knowledge for the comprehension of their detail; and, having stopped with the visitor during his inspection of the Electric Cautery,—in which a platinum wire, raised to red or white, heated by the galvanic current, is employed instead of a heated iron,—we may descend to the

ground floor, and take our seat in Mr. Buckmaster's Food lecture-room. Here we may learn the mysteries of preparing filletted soles and fennel sauce, or study domestic economy by ascertaining how to utilise the bones of the sole just placed upon the operating table. Mr. Buckmaster's little room is sadly disproportionate to his audience, while to our thinking it forms by no means the least important part of the Exhibition; for, although the people whom his discourse will benefit are not likely to hear him, the step is the first in the right direction, and, if supplemented by cheap or free lectures in various parts of the metropolis, would be of incalculable good.

Such, briefly, are the salient points of this year's efforts on the part of the Commissioners, and we think the visitor will find with us that these efforts have provided an excellent illustration of the healthiness of this movement for promoting the welfare of our national industries.

NOTICES OF BOOKS.

Elementary Treatise on Natural Philosophy. By A. Privat-Deschanel. Translated and Edited, with Extensive Additions, by J. D. Everett, M.A., D.C.L., F.R.S.E., Professor of Natural Philosophy in Queen's College, Belfast. Part IV. Sound and Light. London: Blackie and Son. 1872.

This is the last part of Dr. Everett's admirable handbook. The arrangement, which we have previously noticed as methodical in the highest degree, appears in no one of the three other volumes so logical as in this. The comparison and contrast of the vibrations of light and sound are calculated to afford material assistance to the understanding of the phenomena presented successively to the student, especially in the case of the difficult problems of polarisation and interference. Under the head of Acoustics there are considered the production and propagation of sound, its numerical evaluation, modes of vibration, consonance, dissonance, and resultant tones. Under Optics the subdivisions are propagation and reflection, refraction, lenses, optical instruments, dispersion and spectra, colour, the wave theory of light, and polarisation. The chapters on the wave theory of light and the numerical evaluation of sound are particularly worthy the student's attention; and he will find the illustrations as clear, and as fine specimens of wood-engraving, as in the former parts of the work.

A Manual of Recent and Existing Commerce. From the Year 1789 *to* 1872. By John Yeats, LL.D., &c. London: Virtue and Co. 1872.

Dr. Yeats is already well known by his works on the technical and natural history of commerce, the growth and vicissitudes of commerce, &c. The title of the present work is sufficiently self-explanatory. The history is inclusive, and, as far as may be, exhaustive; it is rendered so by the author's terse style and syllogistic method. The preface states the work to be a means of preparation for the higher departments of commerce, or as affording matter for reflection during intervals of repose; that it will assist an intelligent reader in arriving at sound conclusions with regard to the credit of any single state, and aid him in a study of the present or prospective position of our own country, we fully agree. We have especially to draw attention to the history and principles of banking as exemplified in the affairs of the United States and of our own national bank. The particulars of a bank-parlour inspire an ordinary person with considerable awe; but on the perusal of such a work as this the transcendental interest speedily gives place to a deeper respect for commercial integrity and a right appreciation of the demerit of all

that tends to subvert a system of honourable economy, all the reckless venture too often witnessed as the cause of monetary panic. Dr. Yeats's works would be an admirable adjunct to a chair of Commercial Economy; and it is not Utopian to express the hope that at no far distant date colleges will prepare universally for science, commerce, and the arts.

Popular Lectures on Scientific Subjects. By H. HELMHOLTZ, Professor of Physics in the University of Berlin. Translated by E. ATKINSON, Ph.D., F.C.S. London: Longmans and Co. 1873.

PROFESSOR HELMHOLTZ has been known for a number of years in the English scientific world as one of the foremost thinkers of the age, and his admirable Memoirs have from time to time appeared in the "Transactions of the Royal Society," and the "Philosophical Magazine." This is, however, the first time that any of his lectures have been brought within the grasp of the well-informed non-scientific reader. But the book will also be very acceptable to the purely scientific man, for it contains several lectures not before published. Nothing, perhaps, strikes us more in connection with our author than his varied and exact knowledge; as a pure physicist he takes a very high standing; he has done much to develop the now dominant doctrine of the Conservation of Energy; he has worked considerably in the domain of thermo-dynamics; and his acoustic researches are most remarkable and original. Again, he is a good physiologist—indeed he was a military physician in the Prussian service, before he was professor of physiology in the University of Königsberg, and he held a similar professorship in Heidelberg before he was appointed to his present professorship of physics in Berlin. Wherever points of contact have appeared between pure physical actions and purely physiological actions, he has endeavoured to trace the exact nature and course of the concurrent phenomena. His researches on the organs of sight and hearing are of high merit, and receive the admiration alike of the physicist and the physiologist. Add to all this the fact that Prof. Helmholtz is a mathematician; and, most rare of all, that he can clothe his profound generalisations, in whatever subject he may discuss, in most lucid and elegant diction, and the reader has foreshadowed before him what an intellectual feast he may expect from the work we are about to examine.

The lectures have been delivered at various times during sixteen years; one, "On Goethe's Scientific Researches," so long ago as the spring of 1853; another, "On the Interaction of the Natural Forces," in 1854; and the latest, "On the Aim and Progress of Physical Science," in 1869. As to the purport of the lectures, the author says:—"If I may claim that they have any leading thought, it would be that I have endeavoured to illustrate the essence and import of natural laws, and their relation to the mental activity of man. This seems to me the chief

interest and the chief need in lectures before a public whose education has been mainly literary."

We will now glance at the Lectures *seriatim*, premising that the first and second have been translated by Mr. H. W. Eve, of Wellington College; the third by Mr. A. G. Ellis, whose papers on musical subjects in the "Proceedings of the Royal Society" some of our readers will remember; the fourth and seventh by Dr. Atkinson, who is also editor of the series; the fifth by Dr. Tyndall; and the eighth and last by Dr. Flight.

In the first lecture, which was delivered before the University of Heidelberg in 1862, the author traces the connection between the Natural Sciences and other branches of knowledge. He commences by pointing out the extraordinary progress made during the last century in all branches of Natural Science. So long as its development was slight, we cannot wonder that it was not recognised as an educational engine, or admitted as a part of the university curriculum to take up a position side by side with the more ancient subjects:—Theology, Jurisprudence, Medicine. The astonishing activity of research has altered the condition of things. The four elements of the ancients have become sixty-four; the six planets of 1781 have increased to seventy-five; the fifteen hundred stars of the 17th century have become twenty thousand, the position of which in the heavens has been accurately determined; and so in other branches of sciences. Consequently our universities recognise these subjects now far more fully than ever before, and while in the 17th century they were often represented by one or two professors, they are now taught by seven or eight. The disruption between Moral Philosophy and Physical Philosophy may be traced to Hegel rather than to Kant, for the latter based his Cosmogony upon Newton's law of Universal Gravitation, while the former endeavoured to throw into discredit both Newton himself and the whole body of existing Natural Philosophy. Then came an open feud: "the philosophers accused the scientific men of narrowness; the scientific men retorted that the philosopers were crazy. And so it came about that men of science began to lay some stress on the banishment of all philosophical influences from their work; while some of them, including men of the greatest acuteness, went so far as to condemn philosophy altogether, not merely as useless, but as mischievous dreaming." With the Moral Sciences it was the same; they almost ignored the existence of physical science, and often denied it the very name. This opposition, however, was not long maintained; as the Natural Sciences increased in importance, they received more and more general recognition from other sources. Yet when we remember the points of dissonance between the Moral and the Natural Sciences we must admit that a perfect assimilation can never be possible: "while the Moral Sciences deal directly with the nearest and dearest interests of the human mind, and with the institutions it has

brought into being, the Natural Sciences are concerned with dead, indifferent matter, obviously indispensable for the sake of its practical utility, but apparently without any immediate bearing on the cultivation of the intellect." The author is afterwards led to compare the Natural Sciences with the other branches of learning as a means of culture. He distinguishes between the "Experimental" Sciences and the "Natural" Sciences; and asserts the advantage of the former because "they can change at pleasure the conditions under which a given result takes place, and can thus confine themselves to a small number of characteristic instances in order to discover the law." He regards the discovery of the law of gravitation as "the most imposing achievement that the logical powers of the human mind have hitherto performed." The entire discourse indicates great powers of generalisation. Such attempts to define and determine the precise extent of syncretism which shall exist between diverse sciences can only be made by master minds, which shall be excellently exact, and at the same time comprehensive, and such a mind we have in Prof. Helmholtz.

The second lecture treats of the scientific researches of Goethe. It was said of Sir Humphry Davy that if he had not been a great natural philosopher he would have been a great poet. In the case of Goethe, the poet eclipsed the natural philosopher; while the "Egmont" and "Wilhelm Meister," "Hermann and Dorethea" and "Faust" are always remembered in connection with his name, few recognise the fact that he wrote a "Beitrage zur Optik" two years before "Wilhelm Meister." He also wrote on botany and osteology. He introduced into science two important and fruitful ideas:—"The first was the conception that the differences in the anatomy of different animals are to be looked upon as variations from a common phase or type, induced by differences of habit, locality, and food." The second was "the existence of an analogy between the different parts of one and the same organic being." Goethe's theory of colour is open to much criticism, and violent controversies have raged about it.

The third lecture treats of a subject to which Prof. Helmholtz has devoted considerable attention, and which has received at his hands a notable development. It treats "of the Physiological Causes of Harmony in Music," and was delivered in Bonn during the winter of 1857. Since that time the celebrated "Tonempfindungen" has appeared, and we are glad to learn that this work is now being translated by Mr. Alexander Ellis, and that it will soon be published by Messrs. Longmans. In the lecture on Harmony, the author investigates the "foundation of concord." He gives us eminently scientific definitions of *musical tone, pitch, sound, and quality of tone.* The formation, progress, and interference of waves is admirably treated, and the woodcuts relating to this subject are worthy of close study (notably Fig. 2, p. 72). In concluding, the author remarks, "For

the attainment of that higher beauty which appeals to the intellect, harmony and disharmony are only means, although essential and powerful means. In disharmony the auditory nerves feel hurt by the beats of incompatible tones. It longs for the pure efflux of the tones into harmony. It hastens towards harmony for satisfaction and rest. Thus both harmony and disharmony alternately urge and moderate the flow of tones, while the mind sees in their immaterial motion an image of its own perpetually streaming thoughts and moods."

The fourth lecture treats of "Ice and Glaciers," and discusses in some detail the various views of Tyndall and others in regard to the formation of ice, the compression of snow into ice, and regelation.

In the next lecture, which was delivered in Königsberg in 1854, the author discusses the "Interaction of Natural Forces." In this we have an admirable account of the transmutation of the various so-called physical forces, and of their relationship to each other. The connection is clearly and cleverly traced, and is illustrated very happily by examples. Some of us will remember that when in 1798 Rumford boiled water by friction, he remarked that if fuel ever became scarce we could cook our food by transforming mechanical action into heat, as he had then done; we did not, however, know before that "in some factories, where a surplus of water power is at hand, this surplus is applied to cause a strong iron plate to rotate rapidly upon another, so that they become strongly heated by the friction. The heat so obtained warms the room, and thus a stove without fuel is provided." In a town like Bristol, where the rise and fall of the tide is considerable, the amount of heat which might thus be obtained from mechanical sources would be considerable; and if water-mills to produce heat by friction were placed in the Rhone, as it leaves the Lake of Geneva, all the poor of that city might have their food cooked in a public kitchen, in which the heat should be generated by purely mechanical means.

The sixth lecture is entitled "The Recent Progress of the Theory of Vision," and is translated by Dr. Pye-Smith, of Guy's Hospital. The eye is discussed from a threefold point of view: physical, physiological, and psychological; the latter treats of the mental realisation of the changes which take place in the optic nerve. In summarising the conclusions regarding the perception of sight, the author remarks that "the correspondence between the external world and the perceptions of sight rests, either in whole or in part, upon the same foundation as all our knowledge of the actual world—on *experience*, and on constant *verification* of its accuracy by experiments which we perform with every movement of our body. It follows, of course, that we are only warranted in accepting the reality of this correspondence so far as these means of verification extend, which is really as far as for practical purposes we need."

The seventh lecture is devoted to a subject which Prof. Helmholtz has largely contributed to establish and develop—the Conservation of Force. This law, which possesses a great generality of application, although partially recognised by Newton and Daniel Bernouilli, by Rumford, Davy, and others, was first enunciated in its universality by Dr. Julius Mayer, in which work he was ably supplemented by the admirable experimental results obtained by our countryman Joule. The law asserts that the "quantity of force which can be brought into action in the whole of Nature is unchangeable, and can neither be increased nor diminished." The law has been so admirably illustrated and discussed by Tyndall and others in this country, that we need scarcely allude to the details of this lecture. We may mention, however, in passing, the fertility of illustration which the author possesses; among other experiments we notice (Fig. 47, p. 345) a means of producing fire by the simple friction of two pieces of wood after the manner of the savages, but which we have in vain tried to do even by the use of a turning lathe and two pieces of wood differing considerably in hardness.

The eighth and last lecture, on "The Aim and Progress of Physical Science," was delivered in Innsbrück in 1869. In this the author enters into a discussion of various ideas which have—some for a longer, some for a shorter time—been floating about on the confines of recognised science. He pays an elaborate tribute of admiration to the doctrines of Darwin, discusses various questions concerning life; and is led to remark that "the recent progress of physiology and medicine is pre-eminently due to Germany." Yet he is fain to admit that "both in England and France we find excellent investigators, who are capable of working with thorough energy in the proper sense of the scientific methods; hitherto, however, they have almost always had to bend to social or ecclesiastical prejudices, and could only openly express their convictions at the expense of their social influence and their usefulness." This was written ten years ago. We hope Prof. Helmholtz knows how much these things have changed in England even during that short period.

Here, then, we end our notice of a book, which even in its translated form possesses, quite irrespective of the actual science which it contains, a certain charm of style and diction seldom met with in works of this nature, most seldom to be met with at the hands of exact and profound thinkers. We find here our ardent investigator, our original thinker, our profound mathematician, introducing into the most complex subjects a grace of culture and an elegance of expression which it is always satisfactory to meet with, and which indicates the man of great general as well as special knowledge. We find constant quotations from the philosophical poets of Germany; Prof. Helmholtz evidently adores "Faust," is evidently pervaded by a spirit as full of harmony as any of those great sonatas of "the mightiest among the heroes of harmony" Beethoven.

The Life of Alexander von Humboldt. Compiled in Commemoration of the Centenary of his Birth, by J. Lowenberg, Robert Ave-Lallemant, and Alfred Dove. Edited by Prof. Karl Bruhns, Director of the Observatory of Leipzig. Translated by Jane and Caroline Lassell. 2 vols. Longmans, Green, and Co. 1873.

This work is divided into four parts: the first and second, by Julius Löwenberg, treat of the youth and early manhood of Humboldt, and of his travels in America and Asia; the third, by Robert Avé-Lallemant, gives an account of his sojourn in Paris from 1808 to 1826; and the fourth, by Alfred Dove, describes the incidents of the meridian and decline of his life, a time included between 1827 and 1859.

It is difficult in a review to give even a skeleton biography of a man whose life was extraordinarily eventful. One does not know where to begin or where to end. The man possessed a mind of such fertility and such power of thought that he was eminent in almost every subject that he handled, and the mighty extent of his knowledge is altogether surprising. He was in every respect an intellectual giant.

The difficulties of compilation have, in this instance, been considerably increased by the singular modesty of Humboldt. He was unwilling to furnish any letters or other documents which could throw light upon his life and labours. In his will, dated May 10th, 1841, he writes—"I request that my dear relatives and friends will endeavour to prevent the appearance of any biographical notice of me, or laudatory article, in either the *Staatzeitung* or other public journal over which they can exercise any control. I have also drawn up a letter for transmission to the Institute at Paris, requesting that the *éloge* usually delivered upon the death of a foreign associate may be omitted in my case." Since the death of Humboldt several small memoirs have appeared: this, however, is by far the most complete biography of him which exists; the information has been drawn from every available source, and these are numerous, for Humboldt was a great correspondent. His letters are often of great interest and value; among them are thirty addressed to Gauss, thirty to Karsten, and no less than three hundred and thirty to Encke.

Alexander von Humboldt was the son of Major von Humboldt, and was born on September 14th, 1769; in which year also were born Napoleon, Cuvier, Chateaubriand, Canning, and Wellington. He was well educated at home by various tutors, and attended lectures on Philosophy and cognate subjects; he also studied drawing, and the arts of etching and engraving on copper. He was fond of collecting botanical and other specimens, and of classifying them. It is strange that neither Alexander nor his brother William had the smallest taste or liking for music; the

latter actually spoke of it as a "calamité sociale." As a boy Humboldt showed a great desire to travel in distant lands, and books of travel were among his favourite literature. In 1787 he matriculated at the University of Frankfort-on-the-Oder. The scientific world was at this time commencing the period of transition which had originated in the great discoveries of Lavoisier, Scheele, Priestley, Cavendish, and others. A good deal of false science was readily received by the Academies: thus Semler communicated to the Berlin Academy a means of producing gold, by keeping a certain volatile salt in a warm and moist condition for a sufficient length of time. Silberschlag had recently delivered lectures on the sun before the Academy, in which he asserted that—"The sun is really a kitchen-fire, and the spots are clouds of smoke and great heaps of soot; consequently where there is a kitchen-fire there must be meat to roast, such as godless people,—Deists, Universalists, and Atheists,—and the devil is the cook who turns the spit." Many of Humboldt's earlier ideas on Physical Science were obtained by attending the lectures of Marcus Herz, a Jewish physician, and ardent disciple of Kant, who commenced the lectures in his 80th year. Humboldt appears to have been very industrious while at Frankfort. In 1789 he went to the University of Göttingen, staying by the way at Helmstädt, to see Prof. Beireis and his wonderful museum. He gives a curious account of the Professor:—"At home he is always engaged in prosecuting discoveries, and just now, as Crell assures me, he spends sixteen hours a-day in reading on various subjects. Besides the European languages, he speaks Egyptian, Chinese, Japanese, as well as some of the dialects of Northern India, and he read out to me with facility, in German, some passages from a Japanese book, yet many people venture to doubt whether he knows Hebrew! He is, in short, a most extraordinary man, who, with the most profound knowledge of Chemistry and Numismatics, combines the charlatanry of the most cunning juggler. . . . He tells me that he can make corn to grow, that he knows of a tree that bears truffles, that he lives without sleep, and in conversation says every minute that 'he has thought upon that subject for six weeks together without eating or drinking.'"

At this time the University of Göttingen was a celebrated centre of Science, and after Science it was renowned for its teaching of Philology and of Political Economy. Many Germans studied there, and the University has had considerable influence on the development of German thought. The students numbered 812, 405 of whom studied jurisprudence, 210 theology, 104 medicine, and 93 philosophy. Among the students there were two English princes, the Counts de Broglie and St. Simon, and Count Metternich. Humboldt remained only a year at Göttingen, leaving it in March, 1790. In after life he acknowledged that he owed to the University the best part of his education.

During the latter part of 1790 he travelled through England and France, and appears to have made most careful memoranda of everything that struck him in those countries. In the following year he entered the School of Mines, at Freiberg, which had been established in 1766, and was now enjoying considerable reputation on account of Werner's notoriety. He resided here only eight months, and was then appointed "Assessor *cum voto* in the Administrative Department of Mines and Smelting Works," which appointment was offered him "on account of the valuable knowledge, both theoretical and practical, possessed by him in mathematics, physics, natural history, chemistry, technology, the arts of mining and smelting, and the general routine of business."

Space will not permit us to do more than allude to the extremely interesting chapter (p. 161, vol. i.), on the state of society in Weimar and Jena, and the circle of cultivated men into whose midst Humboldt was introduced. Here we find anecdotes of Goethe and Schiller, and numerous extracts of letters from Humboldt and others.

In 1799 Humboldt began his greater travels, and to lay the foundation for his great "Cosmos." He visited Teneriffe, and then went to South America. The celebrated expedition to the Orinoco was commenced in 1800; he afterwards visited, in succession, Cuba, Quito, Mexico, and the United States, returning to Europe in August, 1804, after an absence of five years, during which he had travelled 40,000 miles in South America alone. The travels in Asiatic Russia were undertaken about twenty years later, at the request of the Russian Government.

Humboldt resided in Paris from 1808 to 1826. He originally went there on a diplomatic mission with Prince William of Prussia. He arrived at a time when the First Empire was at the height of its glory, and he entered at once into that brilliant circle of men of genius which had congregated in the capital of France. Here he published the results of his expedition to America in twenty folio and ten quarto volumes, the price of an unbound copy being £400. The work was not altogether a success; in the first place, the extravagant price prevented it from being generally purchased by scientific men; and in the second place, the numerous plates, which had caused the book to be so expensive, were not artistically good, and were quite unworthy of the good artists which then existed. While in Paris, Humboldt numbered among his friends at least two generations of scientific men; among them De Luc, Ingenhouz, Delambre, Laplace, Pictet, Arago, Biot, Gay-Lussac, Thenard, Fourcroy, Vauquelin, Milne-Edwards, Jussieu, Haüy, Brongniart, Guizot, and Elie de Beaumont: a few of these men still remain active members of the Institute, while the very name of Fourcroy carries us back to the science of the last century. During his eighteen years of residence in Paris Humboldt was very

industrious; he frequently read papers before the Institute, and published a number of valuable treatises on various subjects. The remainder of his life, which is regarded as the most important period of it, from 1827 to 1859, was passed in Berlin.

The change of residence was made for various reasons, notably because Humboldt was returning home, and felt that he could there better build up his great work, the "Cosmos;" also because the king desired his presence. Prof. Dove, who writes this concluding portion of the biography, gives an interesting comparison of the Paris with the Berlin of forty years ago. The latter city appears to have been far behind the former, both in size and in everything else which tends to make a city great. The contrast must at first have been painful to Humboldt: he "comments in a spirit of bitterness and well-aimed satire upon the propensity of that 'audacious crew,' as Goethe calls the Berlinese, to pull down everything claiming distinction when the first ebullition of enthusiasm has become exhausted." Rahel used to say—"In Berlin everything loses *prestige*, and is pulled down to the level of mediocrity, if not degraded to insignificance: were His Holiness himself to come to Berlin he would soon cease to be Pope, and become something quite ordinary, perhaps a horse-breaker." In fact, Humboldt had left a magnificent and wealthy city to settle down in a city vastly inferior, in intellect, wealth, and importance. However, he soon resumed his old activity, in spite of duties at court, which must have been sufficiently irksome. During the winter of 1827 and 1828 he gave a course of sixty-one lectures on Physical Geography, The first four of these, in which he gave a general description of Nature, appeared afterwards, in an extended form, as the first volume of "Cosmos." Other of the lectures formed the basis of succeeding volumes of the "Cosmos." When the book was printed, some years later, it was received with great enthusiasm, for it had been long expected, and it was known that Humboldt was the only man who could give to the world such vast generalisations as the subject demanded. "If it be true," says Dove, "that 'man wanders among the departed in the same form in which he leaves this earth,' then, at the name of Humboldt, the image of the author of "Cosmos" would rise before the mind as that of a venerable man, with head inclined and deeply-furrowed brow, bearing upon his shoulders, after the manner of Atlas, the burden of the universe—a strange creation, the full significance of which he only could estimate, since he alone had proved it by experience."

It is, we think, a matter of great regret that the translators have thought it wise to omit the third volume of this biography, which contains an account of Humboldt's scientific labours: the catalogue of his various works (appended to the second volume) has also been omitted. To many of us these will be felt as serious omissions. The work has been carefully translated, and

contains a great fund of interesting matter, not alone directly illustrating the life of Humboldt, but at that same time the character of the society in which he moved, and the times. As such it must be welcomed by all English readers of the "Cosmos."

Principles of Animal Mechanics. By the Rev. SAMUEL HAUGHTON, F.R.S., Fellow of Trinity College, Dublin, M.D. Dubl., D.C.L. Oxon. London: Longmans, Green, and Co. 1873. Pp. 495.

THIS great work cannot receive from us the notice which it deserves; the reviewer of it should be profoundly versed in the higher mathematics, and should be withal a skilled and practised anatomist. The Sciences of Geometry and Anatomy have not been hitherto sufficiently cultivated together. The anatomist who consults this work is staggered at the statement that attention is called to "the problem of the equilibrium of an elliptical muscular dome," and to the use made "of the hyperboloid of one sheet, of Ptolemy's Theorem, and of some curves of the third order;" and the geometer is puzzled by the difficulty of mastering, "*inter alia*," the course and attachments of the muscular fibres of the heart. There is no doubt, however, that the union of these two branches of science has produced, and will produce, results of the highest importance in relation to intellectual progress. Not only is a method pointed out for the investigation of some of the host of problems in Biology yet unsolved, but a new light is cast upon the question of medical education in the future. The University man need not cram his higher mathematics with the idea that if hereafter he joins the profession of medicine they must be forgotten to make way for anatomy and physiology, but he may be assured that his knowledge will serve him well in the special studies of his calling, and very probably in the scientific examinations which in the future he will have to pass. It will be a matter of surprise if Dr. Haughton's work fail to place its mark upon the examination papers of some universities. The Rev. Dr. Haughton is the Newton of the Muscular System, and no cultivated anatomist of this or future time can afford to pass by the study of his book.

The general argument of the work is to establish the validity of the principle of "least action in Nature"—the proposition that in the muscular system of animals there is a *perfect* adaptation of means to ends. The conclusion is irresistible that such adaptation is the result of design.

A large portion of the work is occupied by elaborate calculations of the statical and dynamical work done by man and by animals. The number and kinds of animals examined by the author are very great, and the labour of the investigation must have been immense. Some of the results are of great practical

importance—such are the calculations of the work done by muscles in rowing, climbing, and walking, by the human heart, and by the uterus in parturition. It appears that the oarsman who rows one knot in seven minutes performs a work the rate of which, while it lasts, equals six times that of a hard-worked labourer. The maximum hydrostatical force of the heart of man is nearly the same as that of the horse; the resistance to the circulation imposed by the capillaries varies much in different classes of animals—in the horse this resistance is only half of that which it is in the smaller animals. The daily work of both ventricles of the human heart is calculated to be 124·208 foot-tons; the work done by the heart per ounce per minute is 20·576 foot-pounds, whilst the work of the muscles engaged by the oarsman in a race is but 15·17 foot-pounds: in the one case the effort is continuous, night and day; in the other, the strain is for a short duration only. The greatest energy ever attained by a locomotive equalled only one-eighth part of the energy of the human heart. The calculation of the forces employed in parturition establishes points of high importance. "If ever," the author says, "there was a muscular system produced to effect a specific object, the uterine muscle may be regarded as such." This muscle possesses a force of 3·4 lbs., intended to overcome a maximum resistance of 3·1 lbs. The additional force of the abdominal muscles so raises this figure that on an emergency somewhat more than a quarter of a ton pressure can be brought to bear. The author's words rather discourage the use of chloroform and other anæsthetics in labour; but here his conclusions are rather those of the abstract mathematician and physiologist than of the practical obstetrician, who recognises a very wide range, in various patients, of intensity of suffering and capacity of endurance.

The portion of the work devoted to the consideration of muscular types is also of very high interest. The calculations show the superiority in force of the tiger above the lion; the strength of the latter is about two-thirds of that of the former, and the power of the lioness about one-half that of the tiger. The investigation concerning the Canidæ embraces an elaborate examination of the celebrated greyhound, "Master McGrath." As regards Man and the Quadrumana, some people will be glad to know that the difference between human kind and the gorilla is greater than the differences between the Quadrumana themselves. We quite agree with the author in thinking that over haste has been shown in generalising from purely anatomical data. "The skilful artisan can produce from the same number of wheels and pinions either a clock or a roasting-jack, fulfilling the very different functions of marking time and of roasting meat. An ignorant but intelligent savage, who was shown the interior of these machines, would come to the conclusion that they were very like each other, simply because he would consider only their

superficial resemblances, and would be unable to appreciate the purposes which the machines were intended to fulfil. In like manner, anatomists, from observation of apparent resemblances in the structure of organs, such as the brain (of the specific action of whose parts little or nothing is known), have sometimes, rashly, inferred a greater degree of affinity between various animals than there is any logical ground for admitting." (P. 423.)

We are disposed to take exception to certain of the physiological postulates expressed by the author. Speaking of the transmission of centripetal and centrifugal impressions to and from the encephalon, he says—" The time occupied by the sensitive nerves in conveying the impression to the *optic thalamus*, and by the motor nerve in re-conveying the order of the brain from the *corpus striatum*, is different in different persons." This implies the belief that the optic thalamus is the centre for sensation, and the corpus striatum the centre for motion. In face of the observations of Louget, Brown-Séquard, and especially of Vulpian, we consider that this view cannot now be held. Sensation is experienced by animals from whom not only the optic thalami but the whole of the cerebral lobes have been removed. Again, an animal whose corpora striata have been taken away is able to execute movements when irritated—movements which are not merely reflex.

A few errors of typography and spelling may be corrected with advantage in the next edition. A redundant letter frequently forces itself into the word " development," and a sentence reads that at night in London " the absence of *thoroughfare* in the streets enables the cabmen to drive *fats*."

The author says he brings his work to a close " with some regret," as it has afforded him many pleasant hours of thought and research. We hope that he has *not* brought his great and valuable researches to an end in the present work, but will continue to prosecute the task which, although he confesses it to be a pleasure to himself, is none the less a lasting boon to Science.

Geometric Turning. By H. S. Savory. London: Longmans and Co. 1873.

Mr. Savory has here given to the turner, amateur or professional, a full description of a new geometric chuck invented by Mr. Plant. And it is interesting to learn what may be done with the instrument described in mechanical parlance as a " chuck." Supposing the reader conversant with the beautiful curves produced in geometric turning (for their beauty, although relying upon the greatest simplicity of order, is too complicate for description), it may be stated that if the chuck were arranged for all its loops it would produce 93,312, and at 100 revolutions a minute would take fifteen hours to complete the pattern.

"Such a combination, I suppose," says Mr. Savory, "no one has ever attempted; the general amount of time taken in cutting fiures being from a quarter of a minute to five minutes." But further than this, the reader may gather an idea of the possible intricacy of the curves from the fact that a chuck could be constructed which, "if it made one revolution as the earth does in twenty-four hours, might go on for thousands and perhaps millions of years before it travelled again the same path; it would only be to make all the slides and radius self-acting, and the time when they would recur to the same position would be incalculable." Mr. Savory not only delights in the wonders of the appliance, but what he has to tell of his own progress towards perfection in its uses is rendered valuable by chronicle as well of failure as of success.

The Noaic Deluge: Its Probable Physical Effects and Present Evidences. By the Rev. S. LUCAS, F.G.S. London: Hodder and Stoughton. 1873.

Glimpses of the Future Life. With an Appendix on the Probable Law of Increase of the Human Race. By MUNGO PONTON, F.R.S.E. London: Longmans, Green, and Co. 1873.

THE Bible and Science or Science and the Bible has, not very recently perhaps, become a twofold object of investigation, and the emblem of certain sections of the religious and scientific community. Unfortunately, there is amongst writers on the joint subject a too general feeling of confidence in their strength and their comprehensiveness. Everyone feels that the subject is of exceeding difficulty, yet so interesting that it would be almost as difficult to maintain silence. Emphatically the relation of Religion and Science is not a question which a man of ordinary education is qualified to discuss. The intellect required is one trained at the same time to the observation of particulars and to the regard of generalities; and this without bias. Especially the expounder should be a Hebrew, a Greek, and perhaps an Oriental scholar. He, while an accurate and rigidly logical reasoner, must be capable of appreciating, yet of disregarding, the most delicate metaphor. Add to this the requirement of a knowledge of the natural sciences sufficient to rank the possessor as a scientific man; and is there any cause for wonder that our attempts at a conjoint judgment are so unsatisfactory?

But there is another method by which results may be attained more speedily; and it is that pursued by Mr. Lucas in his consideration "of the Noaic Deluge," as well as by Mr. Ponton in his "Glimpses of the Future Life." In the latter case we have a scientific man, who traces the authority of names into the original Hebrew, and who shows where science and logical argument may be brought to interpret the Bible. Mr. Lucas, on the con-

trary, finds that the Bible may, at least in its account of the deluge, be taken as throwing some light upon certain geological facts, while these facts may serve to confirm or corroborate biblical testimony. These views are undoubtedly those that will afford most definite results. They are based ultimately on the axiom that two truths cannot be more than apparently incompatible. And both our authors are men who have distinguished themselves in the investigation; Mr. Lucas is well known by his work, "The Biblical Antiquity of Man;" Mr. Ponton as the author of "The Beginning, its When and its How," noticed in these pages. Mr. Lucas states that he feels conscious that no difficulties besetting his own solution have been *designedly* overlooked or evaded; and that no *facts* requisite to a just and impartial view of the subject have been omitted or distorted. Nothing, in short, has been *assumed* but the truth of Scripture statement. Now this is as it should be. The scientific layman is as convinced of biblical truth as the biblical layman may be of scientific truth; obviously their only method of detecting error on either side is by *reductio ad absurdum.* But it is an open question as to which may be living in the glass-house, and it is better to continue to work, as our authors do, on the axiom of the negation of incompatibility of truths. Proceeding, then, on this common track, Mr. Lucas considers the deluge miraculous in its origin, and that it could not have been produced by any natural force. That it happened is shown by the evidence of present effect. That the "breaking up of the waters," the quiescent stage and the recession have been attended by well marked, although suppository, geological phenomena. Having conceded that the date of the Deluge admits, even upon Scriptural authority, of the utmost elasticity, it is possible that implements but not bones might be found pertaining to the antediluvian period, while the phenomena of "inundation mud" might be shown to be susceptible of explanation upon a diluvian theory. These are the main points of the work; the reader should exhaust it for himself.

With Mr. Ponton, in his "Glimpses of the Future," we are confessedly more at home, for he brings forward scientific reasoning in support of biblical statement. His treatment of Death and Hades as abstract ideas, the interpretation of "heavens" as meaning atmosphere, are especially characteristic of this author's liberal views. Passing, however, to the Appendix on the probable law of increase of the human race, we find it there said:—"The prevalence of law and order in all the proceedings of the Infinite Mind that rules the universe renders it probable that the multiplication of mankind on the earth may have been regulated by some law which a careful investigation may possibly discover." Having assumed that the present races of mankind have all sprung from the eight persons composing the family of Noah, it appears that twenty-seven reduplications from those

eight individuals would, with a slight addition, amount to the present population of the world. Considered that the limit to the population per square mile of land should be that of France (about 168), and that the thirtieth reduplication would bring up this average density throughout the globe, Mr. Ponton supposes that the limit reached, the number of the earth's inhabitants would thenceforward remain almost stationary. May, then, this reduplication have been governed by some law? The reasoning by which Mr. Ponton proceeds to trace out this law possesses the deepest interest. The results are carefully tabulated, and our readers will find the work well worthy of study.

Elements of Natural Philosophy. By Professors Sir W. THOMSON and G. P. TAIT. Part I. London: Macmillan and Co. 1873.

NATURAL Philosophy, as the good old English term runs, is too often so taught as to place the power of correlation as distant as possible. Indeed, the ordinary method works somewhat in this manner. Professors Sir William Thomson and G. P. Tait have chosen what appears certainly a more philosophic course; for, setting out with Newton's definition that "*mechanics* is the science of machines and the art of making them," and that the science which investigates the action of force is properly termed *dynamics*, we are led to the consideration of force acting in two ways (that is, so as to compel rest or prevent change of motion, and so as to produce or to change motion), as in Statics and Kinetics. It has been usual, in our text-books, to deal first with the laws of statics or the balancing of forces; but evidently the laws of kinetics (or rather of kinematics) present more obvious points to the student than do the laws of statics, which are necessarily subject to the limitation of equilibrium.

Yet this we conceive not the most important phase of progress exhibited in the treatment of the subject. Let the student have acquired his knowledge, let him have commenced his course of original research, there is still one higher step to be made, which, if not gained, will render his results of small worth. We refer to the means of becoming acquainted, by experiment, with the material universe and the laws which regulate it. "In general," to quote our authors, "the actions which we see ever taking place around us are *complex*, or due to the simultaneous action of many causes. When, as in astronomy, we endeavour to ascertain these causes by simply watching their effects, we *observe;* when, as in our laboratories, we interfere arbitrarily with the causes or circumstances of a phenomenon, we are said to *experiment*." To observation, for instances, we owe the data of astronomical, meteorological, and geological science; to experiment the deductions of spectrum analysis, electricity, and

the laws of falling bodies. "Mere observation of lightning and its effects could never have led to the discovery of their relation to the phenomena presented by rubbed amber. A modification of the course of Nature, such as the bringing down of atmospheric electricity into our laboratories, was necessary. Without experiment we could never even have learned the existence of terrestrial magnetism."

These are specimens of the exceedingly beautiful and unique illustrations of our authors. But these again are surpassed by the description of the laws by which the experimentalist should be controlled in the deduction of results. In all cases, to quote further, when a particular agent or cause is to be studied, experiments should be arranged in such a way as to lead, if possible, to results depending upon it alone. Or, if this cannot be done, they should be arranged so as to increase the effects due to the cause to be studied till these so far exceed the unavoidable concomitants, that the latter may be considered as only disturbing, not essentially modifying, the effects of the principal agent. Thus, in order to find the nature of the action of a galvanic current upon a magnetised needle, we may adopt either of these methods. For instance, we may neutralise the disturbing effects of the earth's magnetism on the needle by properly placing a magnetised bar in its neighbourhood. This is an instance of the first method. Or by increasing the strength of the current, or by coiling the wire many times about the needle, multiply the effects of the current so that those of the earth's magnetism may be negligible in comparison. In some cases, however, the latter mode of procedure is utterly deceptive—as, for instance, in the use of multiplying condensers for the detection of very small electromotive forces. In this case the friction between the parts of the condenser often produces more electricity than that which is to be measured, so that the true results cannot be deduced. We thus see that it is uncertain which of these methods may be preferable in any particular case; and, indeed, in discovery, he is the most likely to succeed who, not allowing himself to be disheartened by the non-success of one form of experiment, carefully varies his methods, and thus interrogates in every conceivable manner the subject of his investigations. A most important remark, due to Herschel, regards what are called *residual* phenomena. When, in an experiment, all known causes being allowed for, there remain certain unexplained effects (excessively slight it may be), these must be carefully investigated, and every conceivable variation of arrangement of apparatus, &c. tried; until, if possible, we manage so to exaggerate the residual phenomenon as to be able to detect its cause. It is here, perhaps, that in the present state of science we may most reasonably look for extensions of our knowledge; at all events we are warranted by the recent history of Natural Philosophy in so doing. Thus, to take only a very few instances,

to say nothing of the discovery of electricity and magnetism by the ancients, the peculiar smell observed in a room in which an electrical machine is kept in action, was long ago observed, but called the "smell of electricity," and thus left unexplained. The sagacity of Schönbein led to the discovery that this is due to the formation of ozone.

We cannot for want of space follow our authors through the consideration of the principle of repetition in experiment, agreement, and difference, the use of mathematical theories, and the evaluation of error by the method of least squares, all contained in one valuable chapter of a still more valuable volume. We have fulfilled our duty in presenting it to the notice of the teacher, the taught, and the reading public: to the teacher, because he will find in that which will silence the cry "there is no good text-book of Natural Philosphy;" to the student, because it shows how he should be taught or teach himself; to the reading public, because it will give a clear idea of the beauty and exactitude of the logical method in science.

Science Primers: Physical Geography. By ARCHIBALD GEIKIE, LL.D., F.R.S., Director of the Geological Survey of Scotland, and Murchison Professor of Geology and Mineralogy in the University of Edinburgh. London: Macmillan and Co. 1873.

THIS little work will be found of incalculable value to the elementary student. It will be alike interesting to the general reader, as giving a pleasing description of the phenomena of air, earth, and water. The author asks us to follow him in a country ramble, and read the book of Nature unfolded to us, to learn the relationship of the air we breathe and the earth upon which we live, and to watch with attentive eyes the changes which are continually taking place around us. The details are explained in a simple and comprehensive manner, and throughout the book a desire is evinced to impart knowledge which will be of practical value. It is illustrated by several woodcuts.

Physical Geography. By WILLIAM HUGHES, Professor of Geography in King's College, London, Author of "A Manual of Geography," &c. London: Longmans, Green, and Co. 1873.

THIS is a useful little school-book. It treats of the Earth's natural aspects, phenomena, and productions, in a simple and interesting manner. In the present day increased attention is given to the study of these natural phenomena, and this book will prove of material assistance to the elementary student. Teachers will do well to recommend it to their pupils.

On Coal at Home and Abroad, with Relation to Consumption, Demand, and Supply. By J. R. Leifchild, M.A.

This is a re-publication of three articles contributed to the "Edinburgh Review," with an Appendix supplying information on the subject up to the latest date. The chief merit of this work is, that the large amount of statistical and general information which it affords on its subject is thus brought forward to the time of publication. The first essay, on "Consumption and Cost of Coal," is from the last April number of the "Edinburgh Review." Some of Mr. Leifchild's conclusions are discussed in our article on "The Limits of our Coal Supply." The next paper, "On the Coal-Fields of North America and Great Britain," contains a large amount of valuable statistical and geological information; and the same praise is due to the third essay, on "Fatal Accidents in Coal-Mines," and to the Appendix. Many of the facts here stated are but little known to general, or even to scientific, readers that have not lived in black-country districts. For example, during ten years, the deaths from fire-damp explosions—commonly regarded as the most fatal of the dangers of coal-mining—were only about one-fifth of the total fatal accidents. Those from the falls of the roof and coal—of which we commonly hear so little—reached to about two-fifths. Shaft accidents less than one-fifth, and miscellaneous causes and above-ground rather more than one-fifth. We cannot venture upon any further reference to the closely-packed yet readably-connected facts of this small volume, which we strongly recommend to the perusal of all who are interested in the subject.

PROGRESS IN SCIENCE.

MINING.

FROM the evidence recently given by the several coal inspectors before Mr. Mundella's committee for inquiring into the present state of our coal trade, under the presidency of Mr. Ayrton, we are enabled to glean figures which represent the actual production of coal in Great Britain during the year 1872. The following statistics, showing the output of last year, are of much interest for comparison with the returns of the previous year:—

	Tons.
South Durham..	17,395,000
Northumberland and Durham	13,000,600
Yorkshire	14,576,000
Derbyshire	10,660,000
Lancashire and North Wales	18,363,236
North Staffordshire and Worcestershire ..	6,327,188
South Staffordshire..	10,550,000
Gloucestershire and Somersetshire	7,000,000
South Wales	10,131,725
Scotland	15,383,609
Total	123,386,758

These figures show that, so far from the rise in prices and the disturbed state of the labour market having diminished the production of coal in 1872—as might fairly have been supposed—we actually raised during that year several millions of tons more coal than in 1871.

The progress of some trial-sinkings for coal, in the neighbourhood of Cannock Chase, deserves to be chronicled for the scientific interest attaching to these explorations, and the light which they promise to throw upon the relations between the coal-fields of Staffordshire and Shropshire. At Fairoak, a little north-west of Cannock Chase, a boring has proved the existence of coal-measures beneath the Bunter conglomerates, without the intervention of any Permian rocks. At Huntington, due west of Cannock Chase, a hole is being put down with a diamond-mounted borer, but, at the time we write, coal has not been reached.

The well known trial-sinking at Sandwell Park Colliery is progressing favourably. A second seam of coal has been reached at a depth of 232 yards. This seam is about 11 inches in thickness, and rests on a true fire-clay floor.

In the South of England, the Sub-Wealden boring continues to make way, though not so rapidly as might be wished. A fine bed of gypsum, between 50 and 60 feet thick, has been passed through, and promises to become of much economic value. The specimens which we have seen show that the mineral is a beautifully white fine-grained gypsum, which might evidently be worked with great profit. The bed occurs in the Poundsford series, formerly classed with the lower beds of the Weald, but now believed to be of Purbeck age.

An admirable geological description of the coal-bearing oolites of Brora, in Sutherlandshire, will be found in Mr. J. W. Judd's memoir "On the Secondary Rocks of Eastern Scotland, published in the May number of the "Journal of the Geological Society of London." It is these oolitic coals that have lately attracted so much attention; the Duke of Sutherland having, with characteristic energy, authorised a thorough exploration of the old Brora mines, with the view of again testing their coal-producing capabilities.

A new safety-lamp, constructed on an ingenious principle, has been patented by Mr. Landau. The air required for combustion is contained in a chamber

in the lamp itself, so that it is completely independent of the external atmosphere. A chamber of a capacity of not more than 36 cubic inches, is found sufficiently large to contain a supply of air for twelve hours' use. The products of combustion are carried off and condensed by a bent or coiled tube fixed at the top of the lamp. The patentee recommends that the air should be compressed at the mouth of the pit, and that the condensation pipes should be carried to the top of the shaft so as to avoid the retention of hot gases and vapours in the mine.

A form of rock-drill, known as the Power Jumper, has been patented by Messrs. Brydon, Davidson, and Warrington. This drill is said to be recommended by its simplicity, lightness, and small size; and may be worked with a pressure of steam or compressed air of only 30 or 40 lbs. to the square inch.

Mr. Henry S. Poole, the new Inspector of Mines for Nova Scotia, has presented to the Commissioner of Public Works and Mines an excellent report on "Mining in the Province." This report gives a very encouraging account of the present position of coal-mining.

An account of the mineral resources of India by so competent a geologist, and one so well acquainted with the country, as Mr. W. T. Blanford, necessarily commands attention. Mr. Blanford's paper, recently read before the Society of Arts, does not, however, give a very brilliant view of the mineral wealth of our Indian possessions. The ores of copper, lead, and silver, and the deposits containing gold and diamonds, are really of but small value. Tin-ore, on the contrary, occurs in valuable deposits in the Tenasserim provinces, and salt-mines of great importance are worked in the Punjab. Coal is abundant within a certain area, but the quality is inferior. The deposits of iron ore, however, are, according to Mr. Blanford, destined to be the chief source of India's mineral wealth in the future: these ores, though hitherto but little worked, are extremely plentiful, and in some cases are of very high quality.

A a recent meeting of the South Midland Institute of Mining Engineers, at Wolverhampton, a paper on "Colliery Winding Engines" was read by Mr. S. Watkins. After discussing the most important requirements of a colliery engine, he advocated the use of the direct-acting horizontal engine, with link-motion and reversing excentrics for valve-gearing

Among the papers read at a recent meeting of the South Wales Institute of Mining Engineers, we may mention one, by Mr. J. Snape, on the "Use of Compressed Air Underground for Winding and Pumping."

The Rev. H. Sandford has communicated to the South Staffordshire and East Worcestershire Institute of Mining Engineers a paper on "Mines' Schools in Germany," in which he describes the method of instruction pursued in the schools at Saarbrück and Siegen, and compares it with the education given in this country.

METALLURGY.

During the past quarter the Iron and Steel Institute has held its Annual General Meeting. Mr. Bessemer, the retiring President, has been succeeded by Mr. I. Lowthian Bell, who took occasion, in his presidential address, to lay before the Institute a masterly review of the present position of the iron and steel manufactures of this country. It is pleasing to remark that Mr. Bessemer has liberally placed in the hands of the Council a sum of money for the purpose of founding a gold medal to be awarded annually for the promotion of metallurgical science, either by original research or by literary work.

Among the communications presented to the Institute at this meeting, we may call especial attention to Dr. C. W. Siemens's paper on the "Manufacture of Iron and Steel by the Direct Process." The iron and steel produced by this direct process are said to be of very high quality, and this in spite of the presence of prejudicial ingredients in the raw materials.

At the same meeting of the Iron and Steel Institute, Mr. T. R. Crampton read a paper on the "Combustion of Powdered Fuel in Revolving Furnaces, and its application to Heating and Puddling Furnaces."

The conditions under which highly silicated pig-iron is produced in the blast-furnace have been studied by M. Samson Jordan, who has lately communicated the results of his researches to the French Academy of Sciences. His observations were made at the Heerdt Iron-works, near Dusseldorf. He concludes that the furnace should be worked slowly at a high temperature, and that the charge should be rich in silica and alumina.

Although scheme after scheme has been proposed for the utilisation of blast-furnace slags, which are so terrible a burden to the ironmaster, it cannot be said that any of these schemes have hitherto been entirely successful. Mr. Wood, of the Tees Iron-works, at Middlesbro', has, however, recently invented a machine for crushing blast-furnace slag, until it is reduced to the consistency of sand. Mixed with 8 or 10 per cent of quicklime, this sand when compressed is said to make excellent concrete bricks, which can be used for building without requiring to be burnt. Moreover, it is suggested that the slag-sand may be advantageously used as a manure.

Bricks of slag are also now made by Mr. Woodward, of Darlington. He merely runs the cinder from the furnaces into moulds, and then subjects the bricks to a process of annealing.

It is worth recording that the fusion of platinum in a furnace of powerful draught has been effected by M. Violette. Fifty grammes of platinum, partly in fragments and partly in the state of sponge, were placed in a Hessian crucible lined with plumbago, and subjected for one hour to the heat of the furnace. A perfectly-fused button of platinum, of the same weight as that of the metal introduced, was found at the bottom of the crucible.

In consequence of the great advance in the price of nickel, a correspondent of "The Times," pretty generally known to be Dr. Percy, has called attention to the fact that a substitute for nickel-silver has long been known, though not manufactured. By substituting manganese for nickel, the writer found that an alloy might be obtained, having all the characters of German silver; but though this alloy was known more than twenty years ago its manufacture has hitherto, for commercial reasons, been suppressed. The present price of nickel—consequent upon the great demand for this metal for purposes of coinage, as well as for nickel-silver and for electro nickel-plating—may lead to the manufacture of this manganese alloy in the place of ordinary German silver.

As the contemplated reform in the German coinage is expected to absorb an enormous quantity of nickel within the next two years, it is worth noting that some authorities have suggested that other metals, instead of nickel-alloys, might advantageously be used for the new coins. Dr. Clemens Winkler strongly advocates the employment of pure aluminium for small pieces, and points to the properties of this metal as precisely those which adapt it in an eminent degree for the purposes of coinage.

MINERALOGY,

Every student of mineralogy is familiar with the fine icositetrahedral, or 24-faced, crystals of Leucite, which occur embedded in the lavas of Vesuvius, and were formerly called "white garnets." These crystals are always regarded as characteristic forms of the cubic system—so characteristic, indeed, that the icositetrahedron has been called, in Haidinger's nomenclature, the *Leucitoid.* Prof. Vom Rath, of Bonn, has, however, been led to study these crystals in a new light, and the results of this study show that the mineral must be transferred from the cubic to the pyramidal system.* By observing the direction of certain striæ on the trapezoidal faces of some of these crystals, he found that they were not mere superficial markings, but were really the

* Leonharlt and Geinitz's "Neues Jahrbuch für Mineralogie, U.S.W.," 1873, Heft II., page 113.

edges of twin lamellæ. He has since shown that the common icositetrahedron of leucite is really a combination of the 16 faces of the ditetragonal octahedron with the 8 faces of the primitive octahedron—four of these being above and four below the faces of the ditetragonal pyramid. Leucite is thus brought within the group of those minerals so characteristic of Vesuvius, which crystallise in the pyramidal system—a group which includes zircon, humboldtilite, meionite, vesuvian, and sarcolite.

It has long been known that diamonds of some size occur in the gold fields of California, and Prof. B. Silliman had suggested that they might be found in the heavy sands from the sluices of the hydraulic washings. This suggestion has lately been verified.* A parcel of sand, taken from the tailings in one of the sluices of the Spring Valley Mining Claim, Cherokee, has been subjected to a searching microscopic examination by Prof. Silliman. This examination has shown that the sands abound in beautiful colourless crystals of zircon, associated with crystals of topaz, fragments of quartz, and granules of chromite and titanic iron ore. But in addition to these minerals, there are a few small masses of a very highly refractive substance, believed from its chemical and physical characters to be true diamond.

Bauxite—the mineral which has long been employed in the manufacture of aluminium—is used by Dr. C. W. Siemens for lining the rotary furnaces in which he produces iron and steel direct from the ore. According to Mr. Riley, large supplies of this mineral are likely to be yielded by the deposits of aluminous iron ores in the north-east of Ireland.

Some time ago we had occasion to mention the discovery of some new minerals containing uranium at the Weisser Hirsch Mine, near Schneeber, in Saxony. Dr. Clemens Winkler has published, in a recent number of the "Journal für Praktische Chemie," the results of his analyses of these minerals.

Some confusion has arisen between the two species *amblygonite* and *montebrasite*, but the distinction has been clearly established by Des Cloiseaux. In 1862, a mineral was found at Hebron, in Maine, U.S., and referred to the species amblygonite; in 1870 a mineral was found at Montebras, in France, and named Montebrasite. Now it has since turned out that the original mineral from Montebras is merely amblygonite, but a second species has been found in this locality to which the name of Montebrasite has been transferred; and it is now proved that the so-called amblygonite from Hebron is really this true Montebrasite. The matter therefore stands thus: two minerals are found at Montebras—one called montebrasite and the other amblygonite; the former is also found at Hebron in the United States, and the latter occurs also at Penig, in Saxony. Montebrasite contains lithia but no soda, and also contains a notable proportion of water; amblygonite contains both soda and lithia, but no water.

Axinite is one of those doubly-oblique minerals whose crystalline forms, so difficult to the student, have been carefully worked out by several crystallographers. Herr Hessenberg has lately examined the axinite from Botallack, in Cornwall, and has discovered two forms new to this species. His paper is well worth thoughtful reading, for the sake of his sensible remarks on the use and abuse of crystallographic symbols.

Some observations on the celestine, or sulphate of strontia, of Rüdersdorf, and of Mokattam near Cairo, have been published by Herr Arzruni. He has also analysed specimens of celestine from several typical localities with the view of observing the effect of isomorphous replacements on the crystalline form of the species.

The new species *Pucherite*—a vanadate of bismuth, from Schneeberg, in Saxony—has formed the subject of a careful crystallographic study by Herr Websky. It appears that the forms of pucherite admit of comparison with those of brookite.

Fluor-spar must be reckoned among those crystallised compounds, which, though abundant enough in nature, have not hitherto been artificially prepared

* "Chemical News," May 2nd, 1873, p. 212.

by the chemist. It is therefore worth noting that quite recently Messrs. T. Scheerer and E. Drechsel have succeeded in obtaining crystallised fluoride of calcium.*

We are glad to observe that the excellent series of crystallographic "nets," constructed and published some years ago by Mr. J. B. Jordan, has been lately reproduced at a low price by Mr. Murby, of Bouverie Street. No better means can be recommended to the student of crystallography, in mastering the elements of this confessedly difficult subject, than the construction of a set of pasteboard models from nets or outlines such as are furnished in these sheets.

ENGINEERING—CIVIL AND MECHANICAL.

Water-Lifting Apparatus.—Prall's water-lifting apparatus, designed for raising water into the tender tanks of locomotives, may be considered as an offshoot from the Westinghouse air-break, and by its use the necessity of providing pumping machinery at stations may in a great measure be obviated. The nature of this apparatus may be thus briefly explained:—At or near the bottom of a well is placed a closed box, with a valve on its under side, by means of which it is kept full by the pressure of the water in the well. From the top of this box are two pipes rising up above the ground; at the upper end of the one pipe, which extends to near the bottom of the box, is a branch for conveying water to the tender; whilst the other, which merely enters the box at its upper side, is fitted at the other end with a branch, which, by means of a length of flexible tubing and a union-joint, can be put in communication with a pipe on the locomotive connected to the compressed air-reservoir of the break. This connection being made, the engine-driver can merely, by turning a cock, allow the compressed air to flow from the reservoir to the submerged tank, when it will, by pressing on the surface of the water in the latter, force this water up the rising main, and enable it to be discharged into the tender tank. On a sufficient supply of water having been raised, the cock is shut, the joint with the air-cylinder disconnected, and the compressed air from the submerged tank escaping allows the bottom valve to open, and the tank becomes again charged ready for another operation.

Retaining Walls.—In a paper read before the American Society of Engineers, by Mr. Casimir Constable, it was stated that a retaining wall is stable when the amount of its weight about the point of rotation exceeds the amount of a certain triangular prism of material back of the wall, about the same point—the intersection of the line of rupture of the wall, and the resultant thrust of the prism. Many formulæ and tables for retaining walls are presented for use, without a factor of safety, since walls proportioned therewith—well built and carefully back-filled—have been permanent. Experiments made on a small scale, in which the theoretic conditions were more nearly fulfilled than in practice, show that such walls are stable, and point out the reason why. The problem having been thus solved, a factor of safety may be introduced in the formulæ, which will allow for shocks, irregular workmanship, and uncertain material. The problem was then considered under these several heads:—the angle of rupture, the height of the prism of rupture, and the direction and point of application of the pressure of the prism. It was shown that the prism of greatest pressure is given by the plane which bisects the angle of repose, and that—

$$P = \tfrac{1}{2} w_1 h^2 \tan^2 \frac{i}{2},$$

in which P = the horizontal force which sustains the prism, w the weight per cubic foot, h the height, and i the angle of the prism.

Pneumatic Foundations.—A paper on this subject was read before the American Society of Civil Engineers, at New York, on the 19th of February last, by General W. Sovy Smith. From that paper it appears that the first two bridges on pneumatic pile foundations, erected in the United States, were—

* "Journal für Praktische Chemie," 1873, No. 2, p. 63.

one over the Santee River, on the North-Eastern Railroad, built in 1855; and the other over the Great Pedee River, on the Wilmington, Columbia, and Augusta Railroad, built in 1857. The air-lock used in sinking these piles was invented by Alexander Holstrom. It was a cast-iron cylinder, 6 feet in diameter and 4 feet high, closed at top and bottom by cast-iron plates, through which were man-holes, opening downward for entrance, and bull's-eyes of glass for light. Two goose-neck pipes passed through the sides and bottom—one for introduction of air, and the other for the discharge of water, when it would not escape through the material underneath the pile. A windlass was attached for raising the earth within the pile, all of which was removed by hand. There were four air-pumps set in a single frame. Construction of the pneumatic pile piers for a bridge over the Savannah River, on the Charlestown and Savannah Railroad was begun in the fall of 1859. The air-lock used was 6 feet instead of 4 feet high, and the cylinders were of wrought- instead of cast-iron. In order to overcome certain defects which made themselves apparent, an air-lock was made of less diameter than the pile, so that an annular space was left between the two, in the plate covering the top of the latter, into which bull's-eyes were introduced. Through the side of the air-lock was a pipe or trap, inclined at an angle to discharge readily any material put into it, and arranged for closing at either end; the outer end being closed, the trap was filled with material, the inner end was then closed, the compressed air thus cut off from the air-lock liberated, and the outer end opened, when the material would pass out. By reversing the process the trap was made ready to receive material again. By this means nearly twice as much work was done in the same time as with the Holstrom air-lock. It was soon found that the sandy material through which these piles were sunk could be raised by the escaping compressed air through a discharge-pipe, and delivered outside in a continuous stream; for this the mouth of a flexible tube, fitted to the lower end of a fixed pipe, was thrust into the wet sand, and moved from place to place as the material disappeared. The ratio of work done to that with the whole air-lock, which before was as 28 to 10, now became as 28 to 1.

The Rigi Railway.—On the 29th of April last Dr. William Pole, F.R.S., read a paper, before the Institution of Civil Engineers, on the Rigi Railway. The object of this railway is to convey passengers to the top of the Rigi, there being no carriage-road thither, the only means of ascent having hitherto been either by walking or by horses, or by *chaises à parteurs*. The summit of this mountain is 4500 feet above the plain, and an unusually steep gradient of about 1 in 4 was necessary. The line commences at Vitznau, on the Lake of Lucerne, and is about 4 miles long. The works are mostly formed by cutting and benching on the rocky slope of the mountain; there is one short tunnel, and but one iron bridge over a ravine. The gauge is 4 feet 8½ inches, and the rails are of the Vignoles shape, weighing 34 lbs. to the yard. The ascent is made by means of a rack and pinion arrangement, similar to that proposed and constructed by Mr. Blenkinsop in 1811. The rack is placed midway between the rails, and is formed of two wrought-iron cheek-plates, having wrought-iron teeth inserted between them and rivetted on each side. The pitch of the teeth is nearly 4 inches. The locomotive weighs about 12 tons, and is supported on four wheels. The boiler is vertical, and its axis is inclined to the base line, so as to be nearly upright when running on the steep gradient. The crank-shaft is worked by gearing on the main axle, on which the cog-wheel is placed. The speed on the incline does not exceed 3 or 4 miles per hour, either up or down: the danger of getting off the rails is met by clips fastened to the vehicles, which pass under the rack bar, and would, if the wheels should run off, prevent the carriages from getting away. Clip-brakes are provided which embrace drums on the cogged wheel-axles, so that when these clips are tightened the cogs are held fast in the rack, and thus support the weight of the vehicle, preventing it from running down the incline.

Dynamometers.—A paper has recently been read, before the Franklin Institute, on the Francis Dynamometer, by Mr. Samuel Webber. The principle of the dynamometer is that of obtaining the "horse-power," on weight moved 1 foot per second, by suspending that weight to the end of a steelyard, which,

were it not thus confined, would be free to rotate, and which would describe a circle of which its length is the radius. The first shaft, passed directly through the axis of the steelyard, has fast on it, at one end, the driven pulley and a bevel gear, which forms one side of a compound or box gear. The bevel on the opposite side and the delivering gear are fast on a sleeve or collar, which is caused to revolve in an opposite direction to the shaft by the two intermediate bevels, which complete the compound, and which, being driven by the first gear, rotate freely around the steelyard, when no power is being transmitted, with the exception of such action as is due to the friction of the shaft and gears, which friction is ascertained and deducted in making a test of power. The weight used on the original machine was 1 lb., and the length of the steelyard such as to move the weight at its extremity 10 feet in one revolution, or 1000 feet in 100 revolutions; therefore 1 lb. moved 1000 feet represented 1000 lbs. moved 1 foot. In the improved instrument the steelyard is made the base of calculation, and is graduated in inches and tenths, each inch representing 100 lbs. Starting at $6\frac{1}{2}$ inches from the centre to clear the frame, the graduation is carried out 20 inches or to 2000 lbs., and the weight obtained as follows:—The extreme length is now $26\frac{1}{2}$ inches, thus being the radius of a circle of 53 inches diameter, or 166·504 inches circumference. This is 13·8753 feet described in one revolution, and the weight, to correspond to 1 lb. moved 10 feet, is found to be 0·7207 lb. Now this weight is only one-half of that required, for on the steelyard the action is that of supporting a weight supported at one end, and the weight is therefore doubled, making it 1·4414 lbs., or 1 lb. $7\frac{1}{16}$ ozs. to represent 1000 lbs. raised 1 foot in a second. The poise or slide weight is obtained by a different calculation, as it is to represent 100 lbs. for every inch it is moved, and as a circle of 1 inch radius is 6·2832 in circumference, 100 times that circle is 628·32 inches, or 52·36 feet; and the weight to correspond with 1 lb. moved 100 feet, or 100 lbs. moved 1 foot, is found to be 1·9098 lbs., which, being doubled as before, gives 3·8197 lbs., or 3 lbs. $13\frac{3}{32}$ ozs. nearly for the movable weight. A worm gear on the second shaft drives the clock by a pinion of 100 teeth, and at every 100 revolutions rings a bell. The practical operation of the dynamometer is this:—Having carefully levelled it and secured it to the floor in the proper line of motion, a belt is brought from the driving pulley on the shaft to the first pulley on the dynamometer, and one led from the second pulley on the dynamometer to the main pulley of the machine to be weighed, and the whole put in motion. The action of the bevel gears immediately raises the steelyard, which is then weighted till it remains motionless in a horizontal position. This weight is noted, and also the number of seconds consumed in making 100 revolutions. The belt leading to the machine is then thrown off, and the weight required to balance the dynamometer in motion also noted and deducted from the total weight. The weight thus obtained is divided by the number of seconds consumed, and the result is the number of pounds raised 1 foot in 1 second.

LIGHT.

Mr. Charles Horner has contributed a paper to the "Chemical News" on the "Spectra of some Cobalt Compounds in Blowpipe Chemistry." A spectroscope of *low* dispersive power is essential for seeing distinctly the bands due to these compounds; and since the accurate position of the absorption-bands had to be determined, the author had recourse to the micro-spectroscope, which enabled him to measure them with precision, although a small hand spectroscope, like Mr. Browning's "Miniature" instrument, is very convenient, especially for examining the spectra of hot beads. Mr. Sorby's interference-plate was used as a scale of reference, whilst Mr. Browning's bright point micrometer served as an indicator. When cobalt oxide is added to boric acid, and strongly fused for two or three minutes in the inner flame, using gas as a source of heat, the cold bead possesses a dull blue colour, and is almost opaque. However, by using a powerful light it gives a spectrum of three very faint bands, nearly in the same position as shown in Fig. 2; if, now, 1 per cent of sodium carbonate be

taken up, and the bead treated as before, when cold, the colour is a murky reddish purple, and the spectrum no longer giving the band in the red, but two obscure absorption-bands, very close together, and a slight shading at F, as

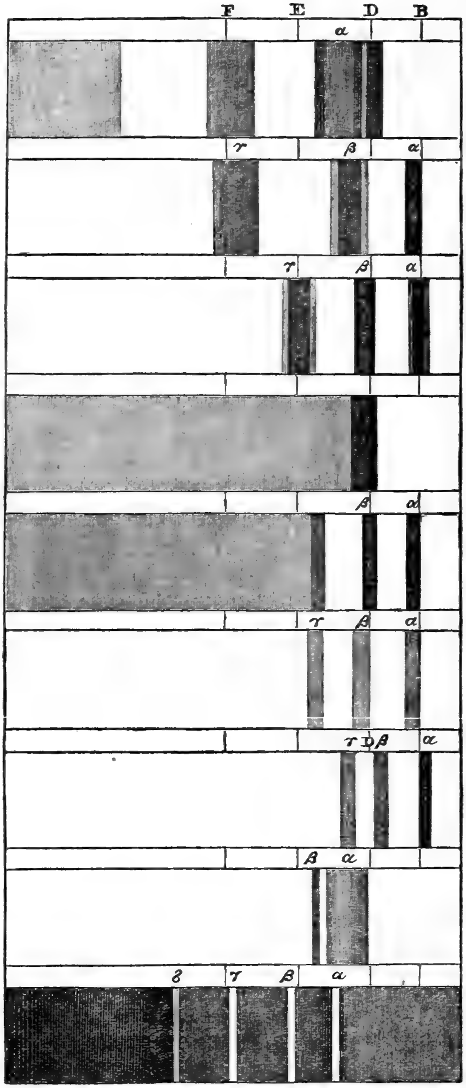


FIG. 1.

FIG. 2.

FIG. 3.

FIG. 4.

FIG. 5.

FIG. 6.

FIG. 7.

FIG. 8.

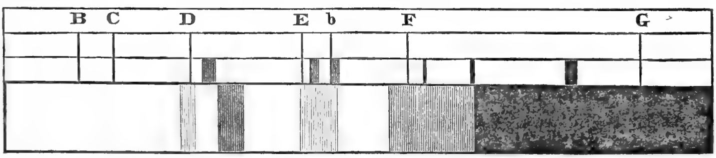
FIG. 9.

shown in Fig. 1. Upon adding 4 per cent of soda the bead becomes quite clear and paler, showing the same spectrum, but while *hot* showing the band α, as in Fig. 2. A further addition of 10 per cent of sodium carbonate causes

the bead to remain a dark purple when *cold*, and exhibiting the complete spectrum of Fig. 2. When 25 per cent has been added a blue bead results, and the spectrum in Fig. 2 changes; the band marked β is perceptibly lowered, overlapping D, as in Fig. 3, but the band γ *remains* as in Fig. 2. If 5 per cent more is taken up, the three absorption-bands are like those drawn in Fig. 3, which represents cobalt dissolved in borax. The bead is now very dark, and requires pressing, whilst hot, between the points of the forceps, to render it sufficiently diaphanous. In these experiments it is necessary the amount of cobalt should be known when added to the weighed boric acid bead, since the determination of the alkali is affected by the quantity of oxide present; *e.g.*, if enough cobalt has not been dissolved, it may require 17½ per cent of sodium carbonate to render the band visible in the red of Fig. 2. Fig. 4 represents the spectrum of a salt of magnesia when moistened with a solution of cobalt. Fig. 5 is the spectrum of the indigo-blue compound of calcium oxide when treated with cobalt. Fig. 6 is the interesting spectrum of the bright blue compound of alumina. The beautiful green compound of oxide of zinc, which is occasionally used as a pigment, and known as Rinman's green, gives the spectrum pourtrayed in Fig. 7. When a thin soda bead is formed with a mere fragment of boric acid, and fused along with a little cobalt, in the outer flame, the bead while hot is of a deep orange-brown colour, turning nearly black and opaque on cooling, but before becoming quite cold rapidly crystallises and turns green. By transmitted light the whole of the blue is cut off and part of the red, while a narrow band is visible at the yellow end of the green. If the bead, however, be submitted to the action of the inner flame, it turns a pale blue while hot, and crystallises on cooling to a pale pink by transmitted light, assumes a lavender tint by reflected light. This unique spectrum of the pink bead is depicted in Fig. 8; and Fig. 9 represents the flame spectrum of boric acid, consisting of four bright bands.

Mr. George J. Warner, F.C.S, calls attention to the peculiarly sensitive character of Wallace's gas-burner. It consists of a hemispherical chamber, into which the gas is introduced through a cone fixed horizontally at a tangent, the position of the jet with regard to the cone being so adjusted that the quantity of air injected by the velocity of the gas at all ordinary pressures is always the proportion required for its perfect combustion. The upper part of the interior of the chamber is lined with wire gauze, and from it issue one or more tubes, at which the gas is burned. At ordinary pressures the flame is of the colour of a Bunsen burner, but with a central cone, clearly defined, of pure green, whether it be turned high or low. But if the gas be reduced below the ordinary pressure on the main, the flame becomes white-tipped, and there is no longer perfect combustion, as in a defective Bunsen. We then find that the flame is sensitive to sound, to all sound in fact, but to high notes particularly.

MICROSCOPY.—Mr. H. C. Sorby has obtained from *Aphides* an orange-coloured fluid, which he names aphidirholeine, giving a very remarkable



spectrum. A narrow band in the orange, having the D line in its centre, another close to the upper edge of the first band of nitrate of didymium, a third a little above E, the general absorption towards the blue commences about midway between *b* and F, and becomes stronger near the fifth band of nitrate of didymium. The band in the orange is a singular and unexpected phenomenon in a fluid of the same colour.

Mr. E. Ray Lankester describes the spectrum of *Stentorin*, the colouring matter of *Stentor cærulius*.* It has two bands, the stronger in the red on the lower side of the C line; its centre is nearer the blue than the red band of fresh chlorophyll. The second band in the green lies somewhat to the blue side of the lower band of fresh blood. Light which has been passed through the thickness of only a single Stentor (which cannot be more than a few thousandths of an inch) is sufficiently affected to show the bands quite sharply. This affords an instance of the value of the micro-spectroscope in cases where only a small amount of coloured material is procurable.

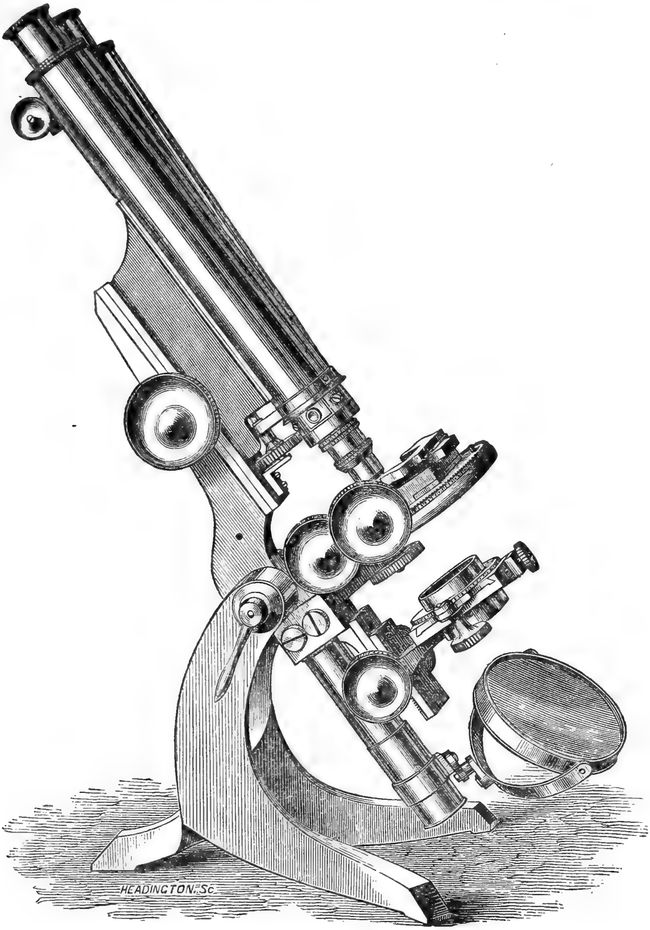
In a paper on a new species of *Callidina*, read before the Royal Microscopical Society,† Mr. H. Davis, F.R.M.S, gives the result of his experiments on the desiccation of this and other rotifers. He has, in common with Pouchet and other observers, arrived at the conclusion that rotifers when completely dried do not revive upon being placed in water. Those which have recovered, and some have done so after gradually heating to 200° F. in an oven, and being kept for a week under an exhausted receiver with sulphuric acid, have been protected, according to Mr. Davis's observations, by a gelatinous secretion which dries over them into a hard thin shell, and effectually secures them from the most complete desiccation the chemist can effect. That such a coating is an effectual protection was demonstrated by the production at the meeting of some grapes coated with gelatine, which had been with sulphuric acid in an exhausted receiver for seven days and nights, and were still perfectly fresh and juicy, while some from the same bunch put by in a cupboard without protection were shrivelled and mouldy.

The lens ruled in squares ‡ by Mr. Ackland for the purpose of making drawings of objects under the microscope has, upon trial, proved successful, the definition being very little affected by it. The markings of the scale of *Lepidocyrtus curvicollis* ("the test Podura") were very well seen under an eighth of Powell and Lealand's new make, magnifying with the eye-piece employed about 800 diameters, scarcely any mischief was done excepting the production of a little colour, the "! markings" were clear enough to permit of a good drawing being made. A convenient size for the squares is 0·05 inch. The drawings are made on ruled paper, several sizes of which, under the name of "sectional paper," are manufactured by Messrs. Letts. Although originally intended for the use of architects, for enlarging and reducing it will be found equally available for microscopical purposes.

The well-known large microscope stand of Mr. Thomas Ross has been remodelled and improved by Mr. F. H. Wenham. The base consists of a single casting, the claws of which are well spread, and give a very firm support to the movable portions of the instrument. The chief variation from the former construction consists in the adoption of the "*Jackson Model*," so strongly advocated by Dr. Carpenter,§ and respecting the rigidity of which there cannot be the least doubt. This is so adapted as not to interfere with the elaborate stage mechanism, which works as freely as in the old form. The "*dove-tail groove*" in the limb is supplemented in its action by a broad flange on the bar carrying the body, which works in close contact with a corresponding plate on the limb, and prevents any chance of a rocking motion taking place. The fine adjustment, as will be seen by the woodcut, is placed in an extremely convenient position, and can easily be reached from either side without the hand being removed from the milled head working the rack motion. The stage and sub-stage arrangements are the same as those in the older form, and need no especial mention. The stand, as now improved, is to be particularly recommended for its great strength and consequent capability of standing hard wear, a quality of considerable importance to the working microscopist. The stiffness of its framing will also recommend it to those who are compelled to carry on high power observations in unquiet situations

* Quart. Journ. Micros. Soc., April, 1873, p. 140.
† Month. Micros. Journ., vol. ix., p. 201.
‡ Quart. Journ. Science, p. 277.
§ Monthly Micros. Journ., vol. iii., p. 183.

and to whom the tremor of the old model was very annoying. The old pattern has been kept in favour solely by the great care taken in its construction by our leading makers, the slightest defect in workmanship being in this form very apparent, and causing a fatal amount of unsteadiness. It affords a remarkable instance of constructive skill overcoming in a great degree the



defects of an originally faulty design. That this form should have been ultimately adopted by the late Andrew Ross is very remarkable, as his older microscopes, as well as those of Mr. Powell, had the body supported by a considerable portion of its length upon a rigid limb.*

Dr. J. G. Richardson communicates to the "Philadelphia Medical Times" some account of the properties of acetate of potash in temporarily preserving tissues during transmission for examination. The process has been principally

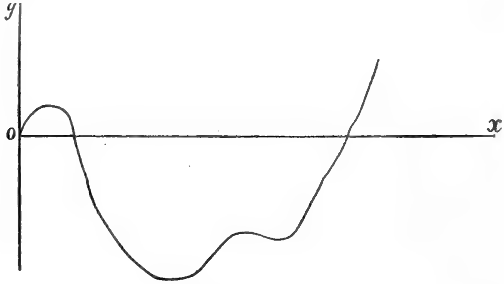
* Penny Cyclopædia, vol. xv., p. 187; article "Microscope," by ANDREW ROSS. A fine specimen of Powell's old construction is in the collection of instruments of the Royal Microscopical Society.

applied to urinary deposits and other pathological specimens. The former, placed in small bottles, arrived in perfectly good condition after a journey of a week in hot weather. Fragments of tumours and other diseased tissues were soaked in a saturated solution for forty-eight hours, until thoroughly imbued with the fluid. They were then pressed, to remove superfluous moisture, and tied up in thin sheet india-rubber or oil silk, and so transmitted by post.

HEAT.

An experiment lately made by Mr. Spence, of Manchester, seems to prove that under certain conditions the diamond is combustible at a much lower temperature than has been hitherto supposed. A South African diamond of the size of a small pea, coated with refractory clay, was placed in a crucible with a mixture of soda and hydrate of lime, and then heated in a muffle for three days and three nights. On opening the mass, it was found that the diamond had entirely disappeared, although the heat had never exceeded a cherry-red.

At a recent meeting of the Helvetic Academy of Sciences M. Dufour gave the principal results of an experimental inquiry into the variations of temperature which occur in diffusion of gases separated by a porous partition. He had studied, among other cases, those of hydrogen and air, of air and carbonic acid; and he distinguishes in these researches between diffusion at constant pressure and diffusion with varying pressure. A porous vessel containing the gas with slower diffusion (air or carbonic acid), and having a very sensitive thermometer applied to its inner surface, was placed in a larger and cloth-covered vessel, in which the other gas (hydrogen or air) was made to circulate. A glass tube through the stopper of the porous vessel communicated in some cases with the open air (constant pressure), and in others with a manometer. The thermometer was observed with a cathetometer. It appears that with constant pressure there is always elevation of temperature on the side of the entering diffusion, and lowering of temperature on the side where the diffusing gas issues from the partition. M. Dufour believes that this change of temperature is not produced throughout the whole gaseous mass, but only at the surface of the porous partition. He points out that at the side of entrance there is condensation, compression, and hence development of heat; while at the other, on the contrary, there is expansion of the gas, and so absorption of heat. With varying pressure the phenomenon is more complicated. The indications of the thermometer are shown by the annexed curve, in which the abscissæ are the times and the ordinates the temperatures.



GEOLOGY.

Stratigraphical Geology.—Dr. Dawson has alluded in a recent number of the "Canadian Naturalist" to the discussion which has reopened on the terms Cambrian and Silurian, and which originated in a difference of opinion between Sir Roderick Murchison and Professor Sedgwick. He agrees with Dr. Sterry Hunt that the Silurian system consists of two groups, which should have distinct names, and that the term Silurian should

be restricted to the Upper Silurian of Sir Roderick, whilst the term Siluro-Cambrian might be applied to the Lower Silurian rocks.

Capt. F. W. Hutton has communicated to the Geological Society a summary of the Tertiary formations of New Zealand, classifying them as follows:—

Pleistocene.	
Pliocene.	Newer Pliocene, or Whanganui group.
	Older Pliocene, or Lignite group.
Miocene.	Upper, or Arvatere group.
	Lower, or Kanieri group.
Oligocene.	Upper, or Hawke's Bay group.
	Lower, or Waitewata group.
Eocene.	Upper, or Ototara group.
	Lower, or Brown Coal group.

Professor Hitchcock gives in the "Geological Magazine" a notice of the precise areas occupied by workable beds of coal in the United States, and he estimates the total to amount to 230,659 acres. This refers only to the coal of Carboniferous age, and excludes other beds of commercial value, which occur in the Triassic strata of Virginia, and in the Cretaceous rocks of the territories west of the Missouri River, in California, Alaska, &c.

Glacial Geology.—Mr. J. F. Campbell, the well-known author of "Frost and Fire," recently communicated to the Geological Society of London a memoir on the glaciation of Ireland. He stated that almost the entire surface of the country consists of glaciated rocks more or less weathered. The polished surfaces are covered in low grounds with drift—unstratified boulder clay being next to the rock, and above it sands and gravels and peat-bogs. The probable dimensions of the ice-engines which work on the surface of Ireland were shown by comparison of glaciers in Iceland, Norway, and elsewhere, with the Irish marks, which indicate ice of equal size. Horizontal grooves at 2000 feet above the sea were instanced as proving ice more than 2000 feet thick, which moved over Ireland into the Atlantic in a south-westerly direction. It was shown that the ice at its maximum probably extended from the Polar Basin to Cape Clear. Mr. Campbell concluded that the later denudation of Ireland was due to glacial and marine action, and that rivers and sub-aërial denudation have done little to obliterate the tool-marks of ice and the sea since the end of the last of a series of Glacial periods.

Origin of Lakes.—The glacial origin of rock-basins occupied by lakes, first promulgated by Professor Ramsay, has received the attention and corroboration of several American geologists. The theory was, however, combated by the Duke of Argyll in his recent Presidential address to the Geological Society of London, wherein he discussed the phenomena of denudation, referring especially to the influence of subterranean and other movements of the crust of the earth upon the denudation of its surface, and disputing the greatness of the denuding effects of glacial action.

The Rev. T. G. Bonney has likewise discussed the theory of the erosion of lake-basins by glaciers, testing it by the Lakes of Salzkammergut, in the North-Eastern Alps. He considered a far more probable explanation to be, that the greater lake-basins were parts of ordinary valleys, excavated by rain and rivers, the beds of which had undergone disturbances after the valley had assumed approximately its present contour. He showed that the lakes were in most cases maintained at their *present* level by drift; and that, while in a region so subject to slight disturbances as the Alps positive evidence for his theory would be almost impossible to obtain, no lake offered any against it, and one, the Königsee, was very favourable to it.

Signor Gastaldi, however, in describing the valley of the Lanzo and other Alpine valleys, pointed to the occurrence of large cirques at heights between 5000 and 10,000 feet. He noticed the various rocks in which these cirques were cut, and expressed his opinion that they are the beds formerly occupied by glaciers, the power of which to excavate even comparatively hard rocks, such as felspathic, amphibolite- and chlorite-schists, he considered to be proved.

Agricultural Geology and Geological Maps.—The relation between Agriculture and Geology is a significant fact, and has been brought prominently into notice in the description of many parts of Great Britain, in the "Journal of the Royal Agricultural Society," and in the "Bath and West of England Agricultural Society." Mr. Topley, F.G.S., has communicated to the first-mentioned Society a paper on the "Comparative Agriculture of England and Wales," in which he gives a table showing the percentage of each crop to the total acreage of each county, and then traces out the relations of geological structure and contour to Agriculture. Classifying the English counties according to their leading features, he remarks that the western part of the country contains the largest portion of high land, and that this higher western land is occupied by the older geological formations. The greatest rain-fall is over this area, and, speaking generally, over other districts the fall is in proportion to the height of the ground. Summer temperature is of great importance; this is highest over the eastern central district. Considered agriculturally, the western counties are characterised by their large acreage of grazing land, whilst in the eastern there is a high percentage of corn land. There is thus a general coincidence between geological structure, contour, climate, and agricultural products. These four classes are stated by Mr. Topley to be of importance in the order here given; each is controlled by the one that precedes it. Agriculture depends mainly on climate, climate mainly on contour, and contour mainly on geological structure.

Agriculture is of course closely related to Geology, in regard to soils, which depend so much upon the underlying rocks; but whilst such extensive systems of draining and manuring are now carried on, the character of the crops is not so dependent upon the natural soil as it used to be. Nevertheless, maps showing the superficial deposits, the gravels, drift clays, and loams, will be of great service to the agriculturist, and it can be little estimated—when glancing at our ordinary geological maps, where these deposits are omitted—what a vast difference their insertion makes. The publication of maps showing the superficial deposits has been commenced by the Geological Survey, in parts of Buckinghamshire, Hertfordshire, Middlesex, and Essex, included in their Sheets Nos. 1 to 7. These drifts or superficial deposits comprise the Boulder clay, and various gravels and brick-earths of different ages, also the clay-with-flints of the chalk districts.

Mr. Topley has given a sketch of the Agricultural Geology of the Weald in the last volume of the "Journal of the Royal Agricultural Society." His paper is accompanied by a coloured map showing the geology of the tract and the prevailing soils.

One of the recently-published maps of the Geological Survey of England is Sheet 64, which includes the county of Rutland and parts of Leicester- and Northampton-shires, and displays, in its geological structure, the Lias, the Lower and Middle Oolitic rocks, and a portion of the Fenland. This area has been surveyed by Mr. J. W. Judd, F.G.S., and until he gave his attention to the elucidation of the Lower Oolitic rocks of this much-neglected country much confusion existed about their classification. The conclusions at which he has arrived have been confirmed by Mr. Sharp, whose papers were referred to in our last report of Geological Progress.

Geological Diagrams.—Two Tables of British Strata, by Mr. H. W. Bristow, F.R.S., Director of the Geological Survey of England and Wales, have lately been published. The one is a classificatory table, showing the subdivision of the rocks and their local modifications: it contains also an account of the geological distribution of the different groups of life, by Mr. R. Etheridge, F.R.S. The other table is a coloured one, showing the relative thickness of the different geological systems, formations, and the minor subdivisions—a feature of great importance in a lecture- or school-diagram.

Mr. Topley, F.G.S., and Mr. J. B. Jordan have been constructing a series of geological models of England and Wales, on the scale of 4 miles to 1 inch horizontal, and 2000 feet to 1 inch vertical. There will be eighteen blocks, each about 25 inches by 17 inches in size, and they will show clearly the physical features and the geology of the country, which will be of great service

in illustrating the connection between the two,—also for military, engineering, and sanitary purposes they will be no doubt exceedingly useful.

CHEMICAL SCIENCE.

With regret we record the death of two distinguished chemists—Drs. Liebig and Bence Jones. Justus Liebig was born in the small German town of Darmstadt, on May 13, 1803, and educated in Bonn and Erlangen. He was originally intended for a pharmaceutist, but having found the means of visiting Paris, and passing some time in the laboratories of the great French chemists who flourished in the year 1823, and, having achieved a success as a chemist, he was at once enrolled by Humboldt in the ranks of the German professoriat, being in 1826 nominated Professor in Ordinary in the University of Giessen, after having for the two preceding years held office as Extraordinary Professor in the same University. Liebig left Giessen in the year 1852, and went to Munich, where he became Professor of Chemistry in the University and President of the Academy of Sciences. In 1845 he had been created a Baron. He died at Munich on the 18th of April. At a meeting of the German Chemical Society, held in Berlin, on April 28th, it was resolved to erect a statue in honour of Liebig, and to invite his pupils, friends, and chemists of all nations to contribute towards the funds necessary for that object. Drs. Warren de la Rue, Frankland, Gilbert, Odling, Stenhouse, and Williamson are members of the Committee, and communications and subscriptions may be sent to Dr. Hugo Müller, 110, Bunhill Row, E.C. The distinguished secretary of the Royal Institution, Dr. H. Bence Jones, died on the 20th of the same month after a long, and latterly, severe illness. His treatise on the early history of the Royal Institution, and his valuable biography of Faraday, are amongst the latest of his works. A movement which was set on foot to get up a testimonial to Dr. Jones, will, in agreement with his own wishes, take the form of a bust to be placed in the Royal Institution.

Curious results followed some of the experiments made upon charred papers and documents, and the examination of books in safes which proved worthless in the great fire at Boston. It was found that what paper makers call poor paper, paper considerably "clayed," stood the test best. Parchment paper, used for bonds and legal documents, shrivelled up exceedingly, and the print blistered so that it could be read when writing was illegible. So it was with the engraved work on notes. The gilding on the account books burned and charred showed out as bright and as clear as when the books were new, which brings up the question if to introduce gilt-edged account books would not be well, on the ground that the gilt would stay the passage of fire to the pages within. Books crammed into a safe so that it was difficult to get them out, suffered considerably less than those that were set in loosely, and in some cases came out from safes, in which everything else was worthless, so far preserved that the figures on their pages could be deciphered. With charred papers, which could not be made transparent by any light whatever used, it was found, after the employment of vitriol, oxalic acid, chalk, glycerine, and other things, that anything that moistened them to a certain stage—to which it was delicate work to get and not to pass—made the lines, words, and figures legible through a magnifying glass. It has been the almost universal experience that lead pencil marks show out all right where ink marks cannot be distinguished. The success of the use of photography has already been noted.

Mr. Elihu Thompson has made the observation that tin-foil, if wrapped about a few crystals of chlorate of potassa, can be made to detonate loudly upon being struck smartly with a hammer upon an anvil, or in a mortar; the phenomenon being precisely analogous to the well-known experiment of triturating sulphur and the chlorate.

In order to ascertain whether ground coffee has been mixed with either roasted corn or amylaceous substances generally, it is only necessary to treat

the powder, first with dilute caustic potassa, and, after filtration and addition of a large quantity of pure water, a solution of iodine is added, whereby the starch is detected.

TECHNOLOGY.

The Boston "Journal of Chemistry" gives the following interesting account of the way in which the natives of East India clean silver articles :—Cut some juicy lemons in slices; with these rub any large silver or plated article briskly, and leave it hidden by the slices in a pan for a few hours. For delicate jewellery, the Indians cut a large lime nearly in half, and insert the ornament; they then close up the halves tightly, and put it away for a few hours. The articles are then to be removed, rinsed in two or three waters, and consigned to a saucepan of nearly boiling soapsuds, well stirred about, taken out, again brushed, rinsed, and finally dried on a metal plate over hot water, finishing the process by a little rub of wash-leather (if smooth work). For very old neglected or corroded silver, dip the article, with a slow stirring motion, in a rather weak solution of cyanide of potassium; but this process requires care and practice, as it is by dissolving off the dirty silver you obtain the effect. Green tamarind pods (oxalate of potash) are greater detergents of gold and silver articles than lemons, and are much more employed by the artisan for removal of oxides and fire-marks.

According to P. Rast so-called German silver may be applied to soldering steel to iron and iron to copper. Borax should be used as a flux, and the German silver granulated as is done for hard brass solder.

Iron may be gilded by first applying sodium amalgam to the iron, which is thereby readily coated with mercury. Next a concentrated solution of chloride of gold is applied to the mercurially coated surface; and lastly, the object is strongly heated, either in a muffle or in the flame of an enameller's lamp.

Common mercurial ointment has been found remarkably efficacious in preventing the formation of rust upon articles of iron and steel, such as gun-barrels.

*** The article in No. xxxviii. of the "Quarterly Journal of Science" on "Atmospheric Life Germs" was written by Mr. W. N. HARTLEY, of King's College, whose name was inadvertently omitted.

THE QUARTERLY

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I. WHAT DETERMINES MOLECULAR MOTION?—
THE FUNDAMENTAL PROBLEM OF NATURE.

CAN the whole phenomena of nature be explained in terms of *matter, motion,* and *force?* Yes, say the materialists; and the number of men of science who agree in this view is not only large, but is increasing daily, whilst a doctrine which would scarcely have been tolerated some years ago is now openly maintained by some of the leaders of scientific opinion. A year ago, Mr. James Croll, of the Geological Survey of Scotland, published some arguments which arrange the problem in an entirely new light; and we are indebted to the courtesy of the author for a private copy of his paper, which was first published in the "Philosophical Magazine," and from which we condense the following arguments.

Hitherto, the chief enquiry has been to ascertain the laws which govern the motions of the molecules of matter, but this is not the grand and fundamental problem. It can be subdivided into—(1.) What produces the change—causes motion? and (2.) What determines or directs it?

The answer to the first question is Force, but the second question—not only the more difficult of the two, but also by far the more important—has been lost sight of or confused with the first. Physicists have devoted almost exclusive attention to the study of the force which takes the path of light, heat, electricity, &c., and upon what does its exertion depend, whilst a vastly more important problem, "What is it that causes the force to act in the particular manner in which it does act?" In other words, "What determines the paths along which it acts?" The great question is, not what gives *existence* to the motion, but what *determines its direction.*

Arguing in this manner, Mr. Croll is led to the proposition that—*The production of motion and the determination of motion are absolutely and essentially different.*

In physics we have been accustomed to attribute every thing to force; force, at least, has always been regarded as the all-important element. This, however, is a mistake; for, as we shall see, far more depends upon the *determination* of force than upon its *existence*, and therefore, *unless force be determined by force, the most important element in physical causation is a something different from force.*

Motion is not only produced, but it is produced in a particular manner and under particular conditions or determinations in regard to time and space and other circumstances. In other words, not only must something produce the motion, but something must determine it also. The causing of, or giving mere existence to the motion, Mr. Croll has called the *production* of the motion. The causing of it to happen in the particular manner in which it does, rather than in some other manner, he calls the *determination* of the motion. It must be evident to every one who will consider the matter that these two things are radically distinct. And they are not only radically distinct, but must be separately accounted for. To account for the mere existence of motion does not account for its happening in one way rather than in some other. It is quite true that the one cannot be produced without the other; we cannot determine motion unless there is motion to be determined; we cannot determine that which has got no existence; neither, on the other hand, can we produce motion without at the same time giving it some particular determination in regard to time, place, or other circumstance. But, although the one cannot be produced without the other, yet they are the result of different agencies; and to assign a sufficient cause for the one does not in the least degree satisfy the mind as to the presence of the other. To account for the motion of a ball does not account for why it moves, say, east rather than west, or in any other possible direction. A force, it is true, cannot act without at the same time acting in some particular way, nor move a body without moving it in some particular direction; but to account for the one does not satisfy the mind in regard to the other. The explosion of the powder within a gun is a sufficient cause for the motion of the ball, but the explosion of the powder is not to the mind a sufficient cause why the ball moves east rather than west, or in any other direction.

The grand and fundamental question then is, What is it that determines or directs the action of the forces concerned in the production of molecular change? The question therefore regards not Law but Cause, unless we use the

term law in an improper sense. Law in physics is not an agent or force, it is simply the *process* or mode of operation —not the force, but the path along which the force acts.

A prodigious number of physical phenomena are perceived to follow as necessary consequences from Newton's grand law, that bodies tend toward each other with a force varying inversely as the square of the distance and directly as the mass of the bodies. But we should reach a higher unity and obtain a deeper insight into nature did we know not merely the empirical fact that bodies do so, but the cause why they do so. It is this which incites in the rational physicist the desire to find out the cause of gravity,

But be all this as it may, whether it be Cause or Law, that is the thing which we are really in search of, every one will admit that the problem of deepest interest is, what causes the molecules and particles of living nature to arrange themselves into organic forms? The problem is not what moves the particles, but what determines or directs the motion—or, in other words, what is the cause of the determination of motion?

What, then, determines molecular motion in organic nature? What determines and directs the action of the forces concerned in the production of specific forms in the inorganic and organic world? Is it a Force? This leads us to Mr. Croll's second proposition, viz.—*The action of a force cannot be determined by a force, nor can motion be determined by motion.*

That the action of a force cannot be determined by the action of a force is demonstrable thus. If the action of a force is determined by an act, then this determining act must itself have been determined by a preceding act; and this preceding act by another, and so on in like manner to infinity. This is evident; for if the act which determines the action of the force exist at all, it must exist in time and space, and must have a determinate existence in reference to time and space, and if so, something must have given it that determinate relation. If it be replied that it was a prior act which determined this determining act, then that prior act in order to give the determining act the proper determination must itself have been already properly determined; and the question again recurs, what gave this prior act the proper determination? If the determination was given by an act still prior, that act must itself have been properly determined; and if so, then there must have been another act preceding which gave it the proper determination, and so in the like manner to infinity. The reason of

all this is perfectly obvious. When we account for the determination of an act by assigning an act, we account for it by means of a something which requires itself to be accounted for in a similar manner.

Hence, be the cause of the determination whatever it may, it cannot possibly be an act or exertion of force.

In a similar manner we can prove that motion cannot be determined by motion. Motion will produce motion, but motion cannot determine motion. A ball A in motion will produce motion in a ball B, but the motion of the ball A will not determine the motion of the ball B, either in regard to direction or to the times of its happening. The particular direction taken by the ball B is not due to the motion of A, but to the particular direction in which A is moving at the moment in which it produced motion in B; so that the direction taken by B must be referred not to the motion of A, but to that something, whatever it may be, which causes A to move in the particular direction in which it moves. In other words, the determinate direction taken by B is not due to the motion of A, but to the *direction* of the motion of A. In like manner it can be proved that the direction taken by A is not due to the motion of some other body (say C), but to the *direction* of that moving body C.

In a similar way we can prove that the particular time at which B begins to move is not due to the motion of the striking body A, but to the *particular time* at which the body A strikes B.

The vague and indefinite idea that the arrangement of the molecules of matter into crystalline and organic forms is due to the action of forces, appears to be implied in such terms in common use as "structural forces," "formative forces," "crystal-building force," &c. It is supposed that if our mental powers were enlarged or strengthened so that we could perceive every thing connected with the forces operating in nature, we should then be able to explain the process by which the organic forms of nature are built up. This, however, is evidently a mistake. Though our acquaintance with the forces of nature were absolutely perfect, the question as to how particles or molecules arrange themselves into organic forms would probably still remain as deep a mystery as ever, unless we knew something more than force.

The mystery is not what are the forces which move the particles, but what is it that guides and directs the action of the forces so that they move each particle in the particular manner and direction required. Force gives motion to the

particles; but we are not concerned about the cause of the motion, but about what *directs* that motion.

When a molecule is to be moved, there is an infinite number of directions in which force may be conceived to move it. But out of the infinite number of different paths, what is it that directs the force to select the right path?

Is it asserted that force is self-directing? This is simply getting into confusion again. What conceivable idea can be attached to a self-directing force? Is force a something which not only acts but determines for itself how and when it shall act? In what conceivable way can force direct its own path? A molecule has to be moved into its proper place in an organic form; a force gives motion to the molecule; but out of the infinite number of possible directions in which the molecule may be moved the force moves it in the right direction. What is that something which thus guides the force? The force guides itself, it is replied. Be it so; but in what way does the force direct or guide itself? What is the nature of that something in virtue of which the force directs its actions? Is it supposed that that something belonging to the force which thus guides and directs its action is itself a force? Does the force direct itself by means of a force? if so, then we are back to our old absurdity of a force determining a force. And if this directing something is not a force, what is it? But if this something is not a force, it follows that there is something else to be known than mere force before we can penetrate the mystery of nature.

The simple truth is, in attempting to account for the *determination* of motion by referring it to a *force*, we are attempting an absolute impossibility. The *production* of motion and the *determination* of motion are two things absolutely different in their essential nature. Force produces motion; but it is as impossible that force can determine motion as that two can be equal to three, or that a thing can be and not be at the same time. The necessity is as absolute in the one case as in the other.

If any one imagines that he can conceive motion as being directed or determined by a force, he will find, on subjecting his thoughts to a proper analysis, that the determination is not due to the force which he imagines, but is due to the *direction* in which his imagined force exerts itself. The determination results not from his imagined force, but from the *way* in which his force acts.

As the distinction between the production of motion and its determination, or between the production of an act and

its determination, is absolute, it must hold equally true in the mental world as in the physical. For example, it is just as impossible to conceive the will being determined by an act, as to conceive the motion of the cannon-ball being determined by the explosion of the powder. It is difficult to say whether in physics or in metaphysics the distinction is of most importance.

What is the cause of determination? What is that something which determines the energies of the universe and guides the motion of the material particles? This is the all-important question, whether as regards life-theories, theism, or evolution.

To a large extent the discussions and diversity of opinion which at present prevail in reference to the mystery of life and the distinction between the organic and the inorganic world take their rise from confusion of ideas regarding the difference between the cause of motion and the cause of the determination of motion. The various theories may be divided into two classes,—the advocates of the one class maintaining that all the phenomena of life, all the changes which take place in organic nature, are the result of purely chemical and physical agencies; while the other party maintain that there must be something more than the ordinary chemical and physical forces at work—in short, that life and organic nature imply the action of a force altogether different from those which belong to the domain of chemistry and physics, and to which the name of "*vital force*" has been applied.

Evidently the vital energies of the plant and animal are derived from the chemical affinities of the food and nutriment which they receive. Vital force is chemical force transformed. The same remark holds true of the mechanical and other physical energies of the body. The energy by which the arm is raised or by which the heart beats is derived from the food. Animal heat is derived from chemical combination.

So far as all this is concerned, the advocates of the physical theory of life are evidently correct. But are they warranted in affirming, as they do, that all the energies of plants and animals are either chemical or physical? Whether such an affirmation be correct depends entirely on the idea which may be attached to the terms chemical and physical.

When the advocates of the physical theory of life affirm that every energy in organic nature is either chemical or physical, they certainly do not mean to include under the

term physical every form of energy which does not, like chemistry, deal with the elementary substances; for if this were their meaning, it would simply be a truism to say that all energy is either chemical or physical. By physical energy they undoubtedly mean the ordinary and known forms of energy manifested in the inorganic world, to which we give the various specific names of attraction, repulsion, light, heat, electricity, magnetism, and so forth. But here we now approach the real question at issue, viz., are these forms of energy along with chemical energy sufficient to account for the phenomena of life and organic nature?

Chemistry and physics are insufficient, because they do not account for the *objective idea in nature*.

Whatever may be one's opinions regarding the doctrine of final causes and the evidence of design in nature, all must admit the existence of the objective idea in nature. We see everywhere not only exquisite order and arrangement in the structure of plants and animals, but a unity of plan pervading the whole. We see, in endless complexity, beauty, and simplicity, the most perfect adaptation of means to ends. The advocates of the physical theory are at least bound to show how it is probable that this exquisite arrangement and unity of plan could have been produced by means of chemical and physical agencies.

Natural selection never can explain the objective idea in nature unless we suppose the selection to be made according to a design or plan. Mr. Darwin has developed a new and most important idea; but his theory can never, from its very nature, explain the mystery of the organic world. There must be a *determining* cause in the background of all natural selection working out the objective idea. This I trust will be rendered more evident when we come to consider determination of motion in relation to final causes.

Mr. Croll now considers the explanation of molecular motion in regard to the *form* of objects.

The objects of nature, as we have seen, are built up molecule by molecule, and are thus the products of molecular motion. Energy is that which moves or transports the molecules in the building-up process; but it is not the mere transport of the molecules, as has been repeatedly shown, which gives to the object produced its form. The form assumed is due, not to the motion of the molecules, but to the determination of that motion—to the way in which the motions are guided and adjusted in relation to one another. It is not the energy which conveys the bricks that accounts for the form of the house, but that which

guides and directs the energy. So far as the form of the house is concerned, it is a matter of indifference whether the bricks are conveyed on the backs of labourers or transported by a steam-crane. In like manner, in accounting for organic forms, we must exhibit not the mere energy which moves the molecules, but that which directs and guides the energy.

But it has been already proved that energy cannot be determined by energy: consequently that which determines energy is not itself an energy. Therefore the thing which we are in search of, which accounts for the order and arrangement prevailing in the molecular movements in nature, is a something not of the nature of a force or an energy.

The question now to be considered is, Can this marvellous adjustment of molecular motions be explained by any thing which is found within the domains of chemistry and physics? The advocates of the physical theory must afford us some explanation of the cause of the determination of molecular motion derived from physics and chemistry, if their theory in reality rests upon a true foundation.

Energy, chemical and physical, accounts for molecular motions in organic nature; but how is it to account for the determination of those motions? If the determinations of molecular motion are to be attributed to these energies, it must be to their modes of operation—the *way* in which the energies are exerted—and not to the mere exertion itself. Suppose that the determinations of molecular motion could be accounted for from the known *modes* of the operation of physical energies. The ultimate problem would then be, What is it that determines those modes of operation? In other words, the problem would resolve itself into this, viz., What is the cause of the determination of physical energies? What is it that directs the operation of those energies?

Molecular physics has made great advance of late years; but it has not made much advance in that particular direction which can be of service in explaining how molecular motion in organic nature is determined. It is thought, however, by the advocates of the physical school that although at present we are unable to explain how organic nature can be built up by the play of the ordinary chemical and physical forces, yet at some future day, when we shall have come to know far more of molecular physics than we do at present, then we may be able to explain the mystery. This is the cherished hope of modern evolutionists, and of the advocates of the physical theory of life. But it is a

mental delusion, a dream which will never be realised. A little consideration might satisfy any one that chemistry and physics will never explain the mystery of nature.

It must be obvious that nothing which can be determined by the comparative anatomist, no biological researches, no microscopic investigations, no considerations regarding natural selection or the survival of the fittest, can solve the great problem of nature; for it lies in the background of all such investigations. The problem is molecular. From the hugest plant and animal on the globe down to the smallest organic speck visible under the microscope, all have been built up molecule by molecule; and the problem is, to explain this molecular process. If one plant or animal differs from another, or the parent from the child, it is because in the building-up process the determinations of molecular motion were different in the two cases; and the true and fundamental ground of the difference must be sought for in the cause of the determination of molecular motion. Here in this region the doctrine of natural selection and the struggle for existence can afford no more light on the matter than the fortuitous concourse of atoms and the atomical philosophy of the ancients.

The next part of Mr. Croll's paper, the publication of which has been unavoidably delayed, will consist of an examination of the arguments which have been advanced by evolutionists in support of their fundamental hypothesis, "that the whole world, living and not living, is the result of the mutual interaction, according to definite laws, of the forces possessed by the molecules of which the primitive nebulosity of the universe was composed."

II. SOME NEW FACTS CONCERNING THE DIAMOND.

WHILST our knowledge of the modes of formation of other gems is so rapidly advancing that the time does not seem to be very distant when the chemist in his laboratory will be able to produce them artificially—if not in large, at all events in microscopic crystals,—the origin and mode of formation of the diamond is shrouded in apparently inexplicable mystery. It is

even undecided whether the diamond is of igneous or vegetable origin, whether its nature is mineral or organic: some diamonds appear to have been soft, as they are superficially impressed by sand and crystals; others contain crystals of other minerals, germs of plants, and fragments of vegetation. Professor Goppert has a diamond containing *dendrites*, such as occurs on minerals of aqueous origin; and there is at Berlin a diamond which contains bodies resembling *Protococcus pluvialis*, and another containing green corpuscles linked together closely, resembling *Polinoglæa macrococca*. Sir John Herschel quotes the case of a Bahia diamond, mentioned by Harting, which contained well-formed filaments of iron pyrites. Messrs. Sorby and Baker have shown that the diamond may contain cavities entirely or partially filled with a liquid, probably condensed carbonic acid, and that the black specks in diamonds are really crystals which are sometimes surrounded by contraction cracks, a black cross appearing under polarised light. Sir David Brewster has likewise pointed out that the diamond possesses strata of different refractive powers. M. Damour states that diamonds sometimes contain spangles of gold in their cavities.

The latest researches published relative to the diamond, namely, those of M. Schrötter and M. G. Rose have furnished results which in some respects correspond neither with each other nor with the facts already known to science. M. E. H. von Baumhauer has instituted some experiments on the subject, special attention being paid to the different states in which the diamond occurs in nature. This talented experimentalist having courteously placed a copy of his research at our disposal, we are enabled to give the most important of his results in these pages. In the first place it was proposed to ascertain the density of the diamond in each of its natural states, afterwards to study its behaviour at high temperatures in the presence of various gases, and lastly to attempt the decision of a point as yet unsettled, namely, the possibility of its transformation by means of heat, into graphite or amorphous carbon. The materials necessary for experimentally pursuing these inquiries were furnished by the liberality of M. Alexander Daniels, the talented manager of the diamond-cutting establishment at Amsterdam, belonging to M. Martin Coster, of Paris.

The condition of a more or less perfect crystal, transparent and colourless, or nearly so, is by no means the only one under which the diamond is found, although for some

time it was only known and sought for in that form. At present, equal attention is paid to irregular fragments of a blackish or greyish colour, occasionally of considerable size, also yielded by the washings of diamandiferous sand, which formerly passed unregarded. These fragments are now carefully collected, and have acquired some considerable value in commerce, where they are known under the name of *carbonado* or carbon. Their aspect is generally that of a rounded mass (although they are sometimes angular) blackish in colour, and presenting a shiny surface as if polished by rubbing. When split, however, their appearance is dull, relieved here and there by a brilliant speck, numerous pores of unequal dimensions being perceptible with a lens. The colour of the split surface varies, being sometimes greyish and sometimes violet. Upon heating in water, considerable evolution of gaseous bubbles ensues; it is therefore necessary, when ascertaining their specific gravity, to boil the fragments for some time in water, to liberate as much as possible the air contained in their pores. Upon treatment with *aqua regia* the solution was found to contain a considerable quantity of iron and a small portion of lime, but no traces of either sulphuric acid or alumina were to be found. Combustion in oxygen produced a small quantity of ash from 0·24 to 2 per cent, according to Rivot. It is to be hoped that *carbonado* may be subjected at some future time to a more minute examination.

Although quite unadapted to ornamental purposes, owing to its appearance, which is totally dissimilar to that of the diamond, *carbonado* cannot be considered as constituting an essentially different condition. Upon examining large quantities of *carbonado* and of diamonds, it is often difficult to decide whether certain specimens should be placed in the category of *carbonado*, which presents no appearance of crystalline structure when seen by the naked eye, or in that of dark diamonds of incomplete and irregular crystallisation. An examination of these numerous varieties has made it evident that between *carbonado* of a simply micro-crystalline texture, and the diamond regularly crystallised in diaphanous octahedrons, there exists an uninterrupted series of intermediate conditions. The true diamond will cleave in a direction parallel with the facets of the octahedrons, while pure fine-grained *carbonado* possesses no such quality, which may nevertheless be found in various degrees amongst the intermediate varieties. It is a remarkable fact that *carbonado*, which in Brazil, and especially in Bahia, always accompanies the diamond, in fragments

whose weight sometimes amounts to several decagrammes, has not yet been discovered at the Cape of Good Hope, although the attention of the diamond seekers has been especially directed towards this apparently worthless substance; it may be therefore inferred that it does not exist in the diamandiferous alluvium of that locality. An examination of two small black fragments, sent from the Cape as supposed *carbonado*, showed that they contained no *carbonado* whatever, but consisted almost entirely of hydrated oxide of iron.

In addition to *carbonado* and to the ordinary diamond, there exists another variety, distinguished by lapidaries under the name of *boart*. The appearance of this is usually spheroidal, translucid—but not transparent—and colourless, or of a greyish tint: it cannot be converted into octahedrons by cleavage, and, moreover, it is much harder than the crystallised diamond, yielding, however, in this respect to *carbonado*. On account of their greater hardness, *carbonado* and *boart* are employed almost entirely in the composition of the powder used in diamond cutting, such powder being greatly preferred by the lapidaries to that formerly in use, which was obtained from well crystallised diamond.

It was thought that crystalline boron, which for some years has been obtainable by artificial means, would equal or even surpass the diamond with respect to its hardness, and might therefore be applicable to the working of diamonds, but the question remains an open one; the high price of the substance in question being opposed to its employment.

As regards the density of the diamond, many data exist, which, however, show many discrepancies. The number 3·5295 was observed by Thompson, at a temperature which is not indicated; M. Halphen found the density of the celebrated diamond, known as the "Star of the South," to be 3·529 at 15° C. M. Schrauf, operating upon the Florentine diamond belonging to the crown of Austria, obtained the number 3·5143, as a mean of two experiments whose results corresponded but little, and after correction for the weight of air, water at 4° C. being taken as unity. A series of experiments were made by M. Schrötter upon different diamonds, among which were always several which were at the same time coloured, and imperfectly transparent, and others which were traversed by fissures. After making the necessary corrections for the weight of the air, the average density of the diamond was found by this gentleman, when compared with that of water at 4° C., to be 3·51432.

Profiting by the facilities offered by M. Daniels, a series of experiments upon density were effected by M. von Baumhauer with all precautions required by this operation.

The water used in the weighing was boiled and cooled in vacuum, the temperature of the water and also that of the air being noted. During the weighing of the diamond in the air upon the pan of the balance, the platinum boat, which was to receive the diamond for its subsequent weighing in water, was suspended to a human hair and already plunged into the liquid, so that the determination of its absolute weight and that of the loss in water should be made under the same conditions, and consequently with the same degree of accuracy. The pressure of the air having varied only between 759 and 761 m.m. during these experiments, it was considered unnecessary to correct for variation of the barometer, such correction exercising no influence upon the result, since the exactitude of this mode of weighing in which there is friction of the hair against the water attains at least the demi-milligramme.

The subjoined table (see page 442) contains the result of these experiments; in the last column will be found the calculated densities D, obtained by adding to the numbers of the last column but one, the corrections arising from one of the weighings having taken place in water at the temperature t_1 and not in water at 4° C., and from the other weighing having been effected in the air at temperature t, and not in a vacuum.

This calculation was performed according to the known formula:—

$$D = \frac{P}{P^1} d_t^1 - \frac{P - P^1}{P^1} \frac{ab}{760(1 \times \beta t)},$$

where a = 0·00129337 grm., is the weight of a cubic centimetre of air at 0° C., and 760 m.m. of barometric pressure, and $\beta = 0{\cdot}00367$ the coefficient of dilatation of the air; no correction having been made for the height of the barometer, $\frac{b}{760} = 1$ was taken.

On comparing the corrected densities in this table, it will be perceived that the purest diamonds possess the highest specific gravity.

No. 1	3·51821
„ 2	3·52063
„ 3	3·51727
„ 4	3·51631
„ 5	3·51934

No.	Quality and Colour of Diamond.	Temperature of Air, t.	Weight of Diamond in Air, P.	Temperature of Water, t^1.	Loss of weight in Water, P^1.	Uncorrected density, $\frac{P}{P^1}$.	Corrected density, D.
1.	Brilliant, nearly colourless (Cape) .	8·0	1·2569	8·0	0·3569	3·5217	3·51812
2.	Brilliant, pale yellow (Cape) . .	8·8	0·7059	8·0	0·2003	3·5242	3·52063
3.	Crude diamond, limpid yellow (Cape)	6·8	6·4830	5·2	1·8415	3·5205	3·51727
4.	Ditto, smaller, quite pure (Cape) .	9·0	1·7095	6·8	0·4857	3·5197	3·51631
5.	Ditto, small black spot in the interior (Cape)	5·5	1·7014	4·9	0·4830	3·5225	3·51934
6.	Ditto, large spot, and fissures (Cape)	9·0	1·1270	7·5	0·3214	3·5065	3·50307
7.	Rough diamond, limpid, consisting of two joined crystals. .	8·0	5·5774	7·0	1·5855	3·5178	3·51436
8.	Spheroid greyish *boart* translucid, not transparent	9·0	1·2808	7·5	0·3649	3·5100	3·50383
9.	White spheroidic *boart* (Cape) . .	14·0	3·9297	12·0	1·1202	3·5080	3·50329
10.	Ditto, smaller (Cape)	14·0	1·2292	12·5	0·3509	3·5030	3·49806
11.	Grey *carbonado* with slight violet tinge (Brazil).	13·0	17·5520	10·0	5·4780	3·2041	3·20053
12.	Greyish black *carbonado* (Brazil) .	13·0	10·3870	10·8	3·1506	3·2969	3·29287
13.	Black *carbonado* (Brazil)	13·0	7·2790	11·1	2·3070	3·1552	3·15135
14.	Spheroidal *carbonado* (Brazil) . .	13·0	2·4165	11·5	0·7215	3·3493	3·34497
15.	Ditto ditto ditto	13·0	1·0965	12·0	0·3418	3·2080	3·20378
16.	Grey, semitransparent, considered *carbonado*, but certainly crystalline	14·0	6·8982	11·5	1·9647	3·5111	3·50652
17.	White, semitranslucid, slightly crystalline, wrongly considered to be *carbonado*	13·0	2·3285	11·7	0·6640	3·5068	3·50215

To take the average of such results, which in the author's opinion is not permissible, the corrected density of the diamond, as compared with water at 4° C., would be 3·51835.

The omission of Nos. 6 and 7 is intentional, those diamonds being permeated with fissures, possibly containing air, which would alter their specific gravity.

It is considered that the density 3·51432, declared by M. Schrötter to be the lowest of his results, is too small; for, amongst the diamonds which he examined, some had blemishes or fissures, whilst in those which were without defect he also obtained much higher numbers; for instance, 3·51869 in a perfectly colourless diamond, 3·51947 in one of pale violet colour. Judging from all known results, M. von Baumhauer believes the density of a pure diamond should not be much less than 3·52.

The figures contained in the Table also show that the density of *boart*, or the globular diamond, seldom exceeds 3·50, whilst *carbonado* possesses a considerably lower specific gravity, being probably a porous diamond, a conclusion confirmed, moreover, by examination with a lens. The higher density found in Nos. 16 and 17 prove that these specimens are not *carbonado*, but some intermediate variety between that and the true diamond.

When shielded from contact with the air, the diamond may be exposed to the highest temperature of our furnaces without undergoing alteration, at least in the case of the colourless diamond; of coloured diamonds more will be said hereafter. The experiment by which this fact is generally demonstrated is conducted as follows: the diamond is placed in a small Hessian crucible and covered with closely compressed magnesia; this is introduced into another crucible which is completely filled up with well pressed graphite, and then the whole is subjected for a long time to the strongest heat obtainable in a porcelain furnace. This experiment was successively repeated by Morren, Schrötter, and others, who ascertained that, notwithstanding the excessive heat to which the diamond was subjected, it underwent no change either in shape or quality; Schrötter, however, remarked that the surface became slightly dull. Similar experiments were made at Berlin by the late Gustav Rose, with the co-operation of Dr. Siemens. A crystal of diamond, enclosed in a piece of dense coke and placed in a plumbago crucible packed with charcoal powder, was heated for half an hour in one of Siemens's regenerative furnaces to the temperature at which cast-iron melts without

undergoing any change whatever. Another diamond, a cut (rose) diamond, which was enclosed in a crucible as before and heated for ten minutes in the furnace to a temperature at which wrought-iron melts, retained its form and the smoothness of its facets, but became quite black and opaque and exhibited a strong metallic lustre. The black portion formed a distinct layer of the thickness of a hair covering the unaltered substance within. These results confirm those of Schrötter, and appear to justify the view that diamond, though it undergoes no change when exposed to the greatest heat of a porcelain furnace or that at which cast-iron melts, is slowly converted at the temperature of molten wrought-iron into graphite.

G. Rose states that some of the specimens of diamond in the Berlin Collection appear quite black by reflected, though translucent by transmitted, light, and that this black substance lying in the little irregularities of the surface is found by its behaviour in fused nitre to be graphite. The relative ease with which graphite and diamond burn was determined by exposing them to the same temperature for the same time, when the following amounts of the three specimens mentioned below were consumed :—

Foliated graphite	27·45	per cent.
Diamond	97·76	,,
Granular massive graphite . .	100·00	,,

The method employed by M. von Baumhauer on examining the action of heat upon the diamond was as follows :—

After previous weighing, the diamond was placed in a small platinum crucible of an elongated and narrow form, similar to those recommended by Mr. J. Lawrence Smith for the decomposition of silicates by chloride of calcium. To enable the operator to observe what took place in the interior of the crucible, it was placed in an inclined position and closed by a thin plate of mica ; an opening was pierced in this plate, through which passed a small thin tube of platinum, soldered at the other end to a glass tube connected with an apparatus which supplied hydrogen dried over sulphuric acid and chloride of calcium. By this means the diamond is surrounded by an atmosphere of dry hydrogen during the experiment. The crucible is heated to whiteness over a gas flame, intensified by a current of air. It was ascertained that the diamond, after exposure for fifteen minutes to a temperature in which it became invisible (that is, where the platinum and the diamond could no longer be

distinguished from each other, from their mutual brilliancy), lost nothing of its weight after cooling, and retained all its transparency and brilliance of surface. The experiment was several times repeated upon colourless diamonds, or those of a pale yellow tint, but always with the same result; in an atmosphere free from chemical action upon it, the diamond may be subjected to a white heat for a considerable time without undergoing any change.

In a superb cut diamond weighing between 6 and 7 carats, the brilliancy of the stone was decidedly increased after the operation. The loss of brilliancy observed by M. Schrötter is a proof, in M. Baumhauer's opinion, that notwithstanding the precautions employed, the diamond had come in contact with the oxygen of the air, or else that at so elevated a temperature a reducing action had been effected upon the magnesia by the diamond, which had then been superficially burnt by the oxygen of that earth.

A diamond which presented to the naked eye an appearance of dirty green, was treated in a similar manner; examination with a lens showed that the colour did not extend to the entire stone, but was confined to small portions, which formed small green clouds in the centre of the mass. After heating to a white-heat in hydrogen, the brilliancy of surface remained as before; the transparency was rather increased than diminished, but the green hue was transformed into pale yellow. Another small diamond, of so dark a green as to approach black, and almost opaque, assumed a violet hue, retaining, however, its brilliancy and becoming much more translucid. A small cubic diamond of light green colour preserved its brilliancy and transparency intact, but lost its colour completely: no difference in its weight before and after the operation could be perceived. Brown diamonds lose most of their colour when heated to whiteness in hydrogen; they generally assume a greyish tint, in all cases the shade is much lighter, and on examination with a lens they appear limpid, with black spots. Diamonds of a yellow tint, such as Cape diamonds almost invariably are, scarcely lose any portion of their natural colour.

Since the last Exhibition at Paris in 1867, opportunity has been afforded of examining a very remarkable diamond belonging to M. Coster. Although almost colourless, upon being heated out of contact with the air (in a magnesia bath) it assumed a deep rose colour, which it retained for some days when kept in the dark; when exposed to the light, however, particularly that of the sun, the colour

rapidly disappeared, but could be restored by again heating it. On examining a rose-coloured diamond, expected by M. Coster to acquire a deeper tint upon exposure to heat, it was found, on the contrary, that the effect of the operation was to deprive it of colour, which it afterwards gradually regained. Several experiments were made by von Baumhauer, in concert with M. Daniels, upon grey diamonds, in the hope that the effect of heat would, by removing the colour, add to their value; but unfortunately the desired result was not achieved, as the diamonds presented after treatment the same greyish aspect as before.

Very different effects are obtained when, instead of heating the diamond in an atmosphere of hydrogen, it is heated in contact with the air. It is unnecessary to employ a white-heat, or to subject the diamond to it for so long a time, in order to render it dull, and consequently opaque; this being the result of positive combustion, which is proved by its loss of weight after the operation. This combustion is, however, quite superficial, as shown by M. Daniels, who found that when re-polished, the diamond recovered completely its transparency and its water; it was, moreover, remarked by M. G. Rose that if the diamond which had become dull was moistened with essence of turpentine, it reassumed its transparency, and retained it as long as its surface continued moist.

The diamond may also be heated in an atmosphere of oxygen, by introducing a current of that gas into the crucible through the small platinum tube before mentioned; in this case the stone attains a vivid state of incandescence, and burns with a dazzling flame long before the platinum crucible has attained a reddish-white heat. In most cases, after the lamp has been withdrawn and the crucible is no longer red-hot, the diamond continues to burn for some time, and presents an appearance of vivid light upon a dark ground. When the diamond is very small combustion may even continue until it is entirely consumed, and it is then seen to dart a more vivid flame at the last moment, like a burning match, the instant previous to extinction. When the stone is of considerable size, the heat produced by combustion is insufficient to maintain it after the removal of the lamp, and it ceases in a few moments notwithstanding the oxygen which continues to flow into the crucible.

Although this last experiment has been repeated several times by these experimentalists, no other result has been observed than tranquil combustion of the diamond; such phenomena as turning black, transformation into coke,

change of the state of aggregation, bubbling up, melting or softening, rounding of corners and angles, were in no case presented to our notice. Once only in experimenting upon an opaque greyish diamond, a few sparks were emitted, but these were evidently due to the presence of some foreign elements incorporated with the whole. Neither did the diamonds burst or split, save in one case, where such was foreseen by M. Daniels: a stone, evidently composed of two diamonds joined together, upon the first application of heat broke with considerable violence into two fragments, each constituting a decided crystal.

It has been asked if the combustion of the diamond in oxygen or atmospheric air is accompanied by flame. M. G. Rose denies this completely, but his mode of operation, namely, by heating the diamond upon a cupel in the muffle of a reverberatory furnace, and drawing it out from time to time for examination, or by heating a thin piece of diamond upon platinum foil in the flame of a blowpipe, was not well calculated to settle the question. On the contrary, by the above method, all that took place in the crucible could be distinctly seen through the sheet of mica, and thus ample evidence was obtained that the diamond, while in a state of combustion is surrounded by a small flame, the exterior envelope of which is a violet-blue, similar to that produced by oxide of carbon in a state of combustion. This is especially the case when the diamond is rather large, when the lamp has been withdrawn and the platinum has ceased to glow: the diamond is then seen upon the black ground of the crucible, brilliant with vivid white light, and surrounded by a zone or aureole somewhat less bright, its exterior edge being a blue-violet colour.

Some highly interesting microscopic observations relative to the dull surface of diamonds which have undergone partial combustion have been communicated by M. G. Rose; he has discovered on them regular triangular markings that resemble those occurring in abundance on the fine crystals from the Vaal River, and recall the faces formed on planes of crystals, soluble in acid, by the slow and imperfect etching action of such a reagent; as, for example, the action of hydrogen chloride on calcite. Like them, these depressions on the diamond bear an exact relation to the crystalline form, and are determined by certain definite faces, their sides being parallel to the edges of the octahedral faces of the crystal. Measurement with the goniometer shows them to belong to the icositetrahedron, the faces of which have not been met with on diamond. These

symmetrically shaped pits can easily be seen by heating a thin plate of boart in a blowpipe flame and examining it under the microscope. By prolonged heating several small triangular pits will often merge into one large one. A crystal of diamond, even when so reduced in size by oxidation as to be only visible with difficulty, continues to exhibit sharp edges and angles. A dodecahedron with very rounded faces but smooth and brilliant surface also exhibited the triangular pits often very distinctly; moreover, it had a brown colour, which was not destroyed by heat, and must, therefore, be of a totally different nature from that of the topaz or smoky quartz.

Several experimentalists, M. Jacquelain amongst others, affirm that at an extremely high temperature, such as is attainable at the focus of a large burning-glass, or between the charcoal points of a powerful galvanic battery, such as 100 elements of Bunsen, the diamond softens, that it passes into an allotropic state, is changed into true coke, capable of employment as an excellent conductor of electricity, and diminishes greatly in density, as much as from 3·336 to 2·6778. It has also been stated, that upon watching through smoked glass the combustion of a diamond under the focus of a burning glass, it was seen to melt, and even to undergo a kind of ebullition.

M. Schrötter informs us that the R. T. cabinet of mineralogy, at Vienna, contains a diamond which was placed under the focus of a burning glass in 1751, by Francis I., the husband of Maria Theresa, and allowed to burn for some time, and that after this partial combustion the diamond, a very limpid well-cut stone, became black both externally and internally.

Clarke, having burnt a diamond in the flame of oxyhydrogen gas, relates that it first become opaque, like ivory, then the angles of the octahedron were rounded, the surface was covered with bubbles, and there remained a globe of metallic brilliancy, which finally disappeared entirely. Silliman, upon burning a diamond upon magnesia, found it turn black and burst, and Murray and Macquer also speak of the diamond turning black under combustion.

Messrs. Rose and Siemens heated the diamond between the two charcoal points of a large magneto-electric machine, the poles being enclosed in a glass cylinder from which air was excluded. During two separate experiments, upon the charcoal becoming incandescent, the diamond exploded into numerous fragments, all of which were black; examination showing, however, that the colouring was wholly superficial,

and that the interior had undergone no alteration: the blackened fragments could be used for writing on paper. From these experiments, and also from the one described above in which the surface of the diamond had turned black after exposure in a crucible of charcoal to heat capable of melting wrought-iron, the conclusion drawn by M. Rose is, that under the influence of excessively high temperatures, the diamond, although preserving its shape, begins to change into graphite, and would probably do so entirely if the heat were sufficiently strong and prolonged for the requisite period.

Opportunity for a repetition of these experiments, not having occurred to M. von Baumhauer, he has not given an opinion upon the behaviour of diamonds at extremely high temperatures; it may, however, be remarked that the blackening which occurs when the diamond is placed in the voltaic arc, may result from transmission of carbonaceous particles from the charcoal poles to the surface of the diamond, which would retain them without the occurrence of any radical alteration. During the employment of the burning-glass, the support upon which the diamond was held might possibly contain matter, which on coming in contact with the carbon of the diamond at so high a temperatue might give rise to reductive phenomena conducive to the formation of the black coating. Something of the kind was observed by M. Schrötter, in an experiment in which the diamond was placed in a crucible in the centre of a mass of strongly compressed magnesia, and moreover folded in a thin sheet of platinum, and then exposed to excessive heat in a porcelain oven. After cooling, the diamond was found to be divested of its platinum cover, which had melted into a globule and adhered to one of its facets. The exterior of the diamond had turned black, whilst its interior was permeated with black dendritic striæ, giving rise to the supposition that a combination of carbon and platina had occurred.

Without having employed the extreme heat attainable in the arc of a powerful galvanic battery, or at the focus of a burning-glass of large dimensions, M. von Baumhauer has, nevertheless, more than once heated diamonds in the oxyhydrogen flame (that is to say, to a temperature capable of melting platinum*) in which the stone emitted a brilliant light, and lost weight rapidly; after the experiment the

* When the points of the platinum which held the diamond were touched by the flame, not only did they melt, but upon examination through smoked glass the platinum was seen to be in decided ebullition.

diamond of course appeared dull, but not the least appearance of blackening was observed, either on the surface or in the interior. Neither was it remarked by M. Jacquelain, when operating with a flame produced by a mixture of oxygen and hydrogen in proportions necessary to form water, or by one composed of a mixture of oxygen and oxide of carbon. The experiment was interrupted several times to examine the diamond, which nowhere presented either brown spots or blackening.

M. Jacquelain considers that perhaps the surfaces of the diamond have been blackened, and that this has disappeared again owing to contact with carbonic acid, and aqueous vapour at a high temperature; in fact, from the considerable friction resulting from the gaseous mixture escaping from a receiver under strong pressure. However that may be, this experiment proves incontestably that the flame resulting from a mixture of hydrogen and oxygen in the same proportion as in water, is incapable of softening the diamond, and that the combustion of this explosive mixture is far from producing the energetic effect of 100 elements of Bunsen. M. von Baumhauer considers that the transformation of the diamond into coke or graphite by means of heat is still to be doubted; nor should it be admitted, until it is quite certain that blackening is not the result of chemical action produced by foreign matter, or by the transmission of carbonaceous particles from the charcoal poles to the surface of the diamond.

To ascertain whether the diamond would be capable, at white heat, of decomposing water, and burning by means of the oxygen contained in it, there was passed over a rough limpid diamond, and also over a cut diamond, a current of superheated steam, in a platinum tube exposed to the heat of a flame of gas urged by a current of air. Although the operation was continued for ten minutes, the diamond was quite brilliant after cooling, and had lost nothing of its weight; proving, that at this temperature at least the diamond suffers no change in an atmosphere of superheated steam.

It is otherwise, however, when the diamond is kept for some time in an atmosphere of dry carbonic acid. A rough stone, weighing 0·1515 grm., was subjected to a white heat for ten minutes in a crucible closed with mica, supplied with dry carbonic acid already flowing, some time before the application of heat; when cooled, the surface of the diamond was dull and its weight decreased by 0·0015 grm. This experiment was repeated with a cut diamond weighing

0·6095 grm.: when withdrawn from the crucible it had become quite dull, with the exception of two facets which had preserved their brilliancy, but were tinged with iridescent colours; the carbonic acid current had exerted upon them a comparative cooling action; the stone had lost about 2 milligrammes. This proves that the diamond is capable at a white heat of decomposing carbonic acid, and of combining with its oxygen, but the action is very slow. This decomposition had already been perceived by M. Jacquelain, although his mode of operation was uncertain in his results. A receiver was employed with two openings, and filled with carbonic acid; one opening communicated with a tube, at whose extremity the oxyhydrogen gas was burnt; through the other was introduced the diamond supported on a piece of pipe-clay. In this experiment the diamond was consumed rapidly, but especially by the oxygen of the oxyhydrogen mixture, no trace of blackening being perceptible.

III. ON COMPARATIVE VEGETABLE CHROMATOLOGY.

By H. C. Sorby, F.R.S., &c.

IN an article on the various tints of autumnal foliage, published in this journal for January, 1871,* I described a number of the leading kinds of colouring matters found in the higher classes of plants. Since then I have studied them far more thoroughly; so that what I said must be looked upon as a mere commencement of a subject which has now become so extensive, and includes special modifications of so many branches of science, that it appears desirable to give some single name to the whole. It is for this reason that I have called it *chromatology*. In this former paper my chief object was to describe the cause of the production of the various tints of leaves when they fade. This is mainly the result of chemical changes taking place when the leaves are dying or dead, which correspond very closely with what can be imitated artificially by acting with various reagents on the dead materials extracted from the living plants. On the contrary, my object now is to

* New Series, vol. i., p. 64.

describe the different coloured substances formed by the constructive energy of living plants, or changes that take place in them whilst they are still portions of living organisms, which changes can be imitated very imperfectly, and I shall only incidentally notice alterations which occur after the plants are dead.

When first I commenced the study of the colouring matters, I was very well contented to confine my attention to those which occur in relatively large quantity in flowers and green leaves, or give striking and well-marked spectra. On extending my researches to fungi, lichens, and algæ, I soon found that the more abundant substances were very different in different classes of plants; and on making more careful comparisons, I found that some of the colouring matters which occur in a relatively large quantity in one class are often not really absent from others, but occur in relatively small amount. This led me to discover that the coloured solutions obtained from green leaves are even more complex than had been supposed, and that, independent of those soluble in water, they contain normally no less than six or seven coloured substances, perfectly well distinguished by their optical and chemical characters. Having determined the chief coloured constituents of the leading classes of plants, I drew up a rough table, showing their distribution through the great groups of the vegetable kingdom, and saw at once that there was such a striking connexion between the general organisation of plants and the character of the colouring matters contained in them, that it was desirable to explore the question as completely as possible. This inquiry would have been very difficult, if not impossible, if I had not been able to contrive fresh means for separating or otherwise recognising the different constituents of complex mixtures.

Many of the most important coloured substances met with in plants are insoluble in water, but soluble in bisulphide of carbon and in alcohol, but the relative facility with which they are dissolved by these two reagents differs very much. When dissolved in spirits of wine of the usual strength, and the solution agitated with excess of bisulphide of carbon, the whole of some of them is carried down in the bisulphide, whereas the whole of some other substances is left in the alcohol if it be strong, but more and more is carried down in the bisulphide when the alcohol is more and more diluted with water. The result is that some substances can be separated perfectly, and others only partially; but by agitating the solution in spirit with excess of bisulphide,

separating the alcoholic solution, and repeating the process over and over again, with the addition of a little water each time, a comparison of the spectra of the different portions thus fractionally separated will often suffice to show whether the original coloured solution was or was not a mixture, and the extremes are sometimes different substances, in a more or less pure state. Of course if the original had not been a mixture, such a difference would not occur, unless some decomposition took place, which could easily be detected. There are, however, cases where different substances cannot be separated in a satisfactory manner by such means, and it would be almost impossible to study comparative vegetable chromatology successfully, if light could not be made use of as a reagent. On exposing to the direct rays of the sun solutions of different colouring matters in bisulphide of carbon or other solvent, some are rapidly decolourised, usually, but not always, without the intermediate production of any new coloured substance, whilst others fade very slowly, some being changed by one kind of light, and some by others. This decomposition usually depends upon the presence of both air and light, and does not occur in the dark when air is present, or in tubes quite free from air when exposed to the sun. The result of this difference in the behaviour of different substances is that, though they cannot in some cases be separated by chemical means, one may be entirely destroyed by exposure to the open sun, or to particular rays which pass through coloured glasses, whilst sufficient of the other remains unchanged to show its characteristic properties in a satisfactory manner. By combining the above-described method of fractional separation with this kind of photo-chemical analysis, it is often easy to unravel very complicated mixtures; and I do not think that anyone who had not tried this system of investigation would be prepared to find how much may be effected by such simple means. By adopting these several methods, it is not only possible to detect a comparatively small quantity of the more important constituents in complex mixtures, but also to determine their relative amount in different cases. This kind of comparative quantitative analysis is of very great value in the present subject. It does not consist in ascertaining the relative weight of the different colouring matters in any one specimen, like the ordinary sort of quantitative analysis, but in determining the relative quantity of each colouring matter in two or more different specimens. In those cases where the constituents can be more or less perfectly separated, the

relative quantity of each kind can be easily determined by having the solutions in two tubes of equal diameter, and diluting one or both until the depth of colour is the same, or still better, until the spectra exactly correspond when compared side by side. The relative amount of each is then known by measuring the length of the columns of solution in the tubes. It is also often easy to ascertain the relative amount of more than one constituent in similar solutions; for the absorption-bands of one may first be made equal, and then those of another, measuring the relative volumes when the solution of each colouring matter is thus found to be of equal strength. For the future I intend to try to carry out this sort of analysis by means of standard solutions, sealed up in tubes free from air; and, if they remain permanent, the relative composition of mixed solutions could be determined without actually comparing them together, which would make it possible to ascertain the changes that take place in plants at different seasons of the year, and to otherwise develope the subject to a far greater extent than heretofore. I have also lately adopted another method, which makes it to some extent possible to dispense with such comparisons. I endeavour to determine the relative proportion of the various coloured constituents in terms of the amount of light absorbed by each. When this is in nearly the same part of the spectrum, the comparison can be made with considerable accuracy; but when it is in very different parts, only approximately, but yet in such a manner as to yield far better results than any other method.

In applying these principles, of course there are many questions of detail, depending on particular circumstances; and what I have now described must be looked upon merely as such a general account of the methods I have adopted as seemed to me desirable, since otherwise the possibility of determining some of the facts might have appeared doubtful. I now, therefore, proceed to the consideration of the subject more immediately claiming attention.

Comparative vegetable chromatology may be divided into two principal parts, viz., that in which we compare leaves or fronds of the same kind of plant growing in different conditions, in order to learn the effects due to external influences, and that in which we compare different plants growing in similar conditions, in order to learn the effects due to internal organisation. Some of the effects of a difference in the amount of light are well known. When it is almost absent, the leaves are yellow and pale, owing to chlorophyll and some other colouring matters not being

properly developed; but I have found by careful comparative quantitative analysis that, when plants are exposed to more light than is requisite for their healthy growth, the amount of chlorophyll and other colouring matters is diminished sometimes to even one-third of the maximum quantity. I have also found that, when a leaf is partially covered up and screened from the light, the amount of chlorophyll increases in the shaded part. In the case of a leaf of *Aucuba japonica*, chosen for the experiment because it is much influenced by light, the increase was no less than at the rate of two per cent per diem. Chlorophyll separated from the leaves is rapidly decomposed by light, and it could scarcely be supposed that a similar change would not to some extent occur in the living plants. In fact the power with which it then resists such a change seems to require special explanation. The general connexion of all the facts I have observed leads me to conclude that some, if not all, the coloured constituents of growing leaves, like the constituents of the bodies of animals, are in a constant state of transformation, new being formed and the old destroyed, the apparently uniform composition being due simply to the establishment of an equilibrium, which remains nearly the same when the conditions are the same, but is very soon changed when they are altered. This supposition explains in a satisfactory manner many facts which would otherwise be unintelligible, and probably one result is that the endochrome * is thus constantly maintained in a young and vigorous condition. According to this view of the subject we may suppose that, in the above-named case, when the amount of chlorophyll apparently increased at the rate of two per cent per diem, the relative increase was due, not to more being developed when the light was excluded, but to more being decomposed in a corresponding portion of leaf left exposed to the sun. The equilibrium of the constituents was thus partially changed from that found in leaves when growing much exposed to the sun to that of leaves growing in the shade.

On comparing the relative amount of the other constituents of various plants, when more or less exposed to the sun, I have found that equal weights of the leaves or fronds contain almost the same amount of those colouring matters which are the least changed by the action of light, and that the relative quantity of the others in those leaves

* I think it would be found very convenient to adopt this term as the name for all kinds of simple or complex coloured substances found in the cells of plants.

exposed to the sun, decreases in the same order as they are more and more rapidly decomposed by the action of light, and in proportion as the leaves or fronds are exposed to more and more light. There is thus established a sort of equilibrium, varying with these different conditions, and easily explained, if we suppose that the different coloured substances are being constantly formed by the internal constructive energy of the plant, and constantly decomposed in varying proportion by the destructive action of the oxygen of the atmosphere, intensified by the influence of light. There are, however, well-marked exceptions to this rule, which require us to suppose that the constructive force varies qualitatively as well as quantitatively, when it is much reduced by the absence of light or other causes, so that some of the different compounds are formed in very different proportions. The development of fructification also sometimes produces a certain amount of alteration, as though the colouring matter formed in the organs of reproduction were abstracted from the fronds. In the case of the lichen *Peltigera canina*, when it grows in a very damp and shady situation, there is a greater relative deficiency of certain colouring matters, which I have named *lichnoxanthine* and *orange lichnoxanthine*, than seems likely to be due to the decomposing action of light on the other constituents of the specimens more exposed to the sun, and the relative amount is again decreased by much more exposure. On the whole it appears more probable that the deficiency is mainly due to their imperfect development when there is too little or too much light for the healthy growth of the plant, and this fact is of much interest, when we know they are the characteristic colouring matters of the fructification, and that it is imperfectly, or not at all, developed in very exposed or in very shady situations, perhaps because these requisite substances are not formed in sufficient quantity. I have also found that there is a most remarkable alteration in the relative amount of the different coloured substances characteristic of *Oscillatoriæ*, when the light is very feeble, evidently due to the weak constructive energy, as will be more fully considered in the sequel.

Having thus learned what is the character and the extent of the changes produced by varying conditions on the colouring matters found in the same, or in closely allied, species of plants, we are in a better position to understand the variations corresponding to the difference in the general organisation of different classes, and to distinguish and

eliminate the effects due to special conditions. The facts described above clearly show that, if we wish to ascertain what changes depend on a difference in organisation, it is necessary to compare normal specimens of each class, growing as nearly as possible in similar circumstances; though, at the same time, it is very desirable to determine what is the effect of different conditions.

In studying comparative vegetable chromatology, it is requisite to distinguish between fundamental and accidental colouring matters. There is the same sort of difference in the case of animals. The hæmoglobin of the blood, and the colouring matters in the bile are, as is well known, of such great physiological importance, that they are essential to the healthy life of the higher classes of animals, whereas the colouring matters in the hair or feathers are of only very indirect utility. In a similar manner the higher classes of plants cannot permanently grow without the colouring matters belonging to the chlorophyll and xanthophyll groups, whilst the various red and blue substances belonging to the erythrophyll group may be present or absent without materially interfering with the growth of the plant, and are either of no use, or only very indirectly advantageous, as for example, in attracting to the flowers the insects instrumental in causing fertilisation. At present it is impossible to decide whether certain kinds of colouring matters are or are not essential to the growth of particular plants, or whether they may not be necessary for some classes, and present in others like those organs of animals which, though requisite for some classes, are only rudimentary and of no use to others. Some, also, may be only constant products. The whole subject is, indeed, only in its infancy; many fundamental questions remain to be decided, and for the present we must be content with having obtained a clue to a kind of research which promises to throw a new light on such inquiries.

It is very common to find that accidental colouring matters are much more conspicuous than some that are probably of great importance. Thus, for example, the crimson-coloured substance which is developed in the leaves of certain varieties of the beech, is so very conspicuous, and disguises the other colouring matters so much, that perhaps few persons would imagine that the normal amount of chlorophyll is present, and yet this is easily proved by comparing the spectrum of a very red leaf, growing where much exposed to the sun, with that of a green leaf, growing in a very shady place on the same tree, the absorption-band

of the chlorophyll being almost equal. This red colouring matter is probably a product of the decomposition of chlorophyll, due to the action of light, when the leaves are in a peculiar low state of vitality. The result of such a predominating influence of what may be called accidental substances is that mere *colouring* is often of very little general significance, even in distinguishing closely allied species. This is, however, quite intelligible, since comparatively small special differences in the constitution of the individual plants may suffice to alter the character of the accidental colouring matters, especially in organs like the petals, which are not essential for the life of the individual plant. In fact, by artificially lowering the constructive energy, by screening flowers from the light, I have succeeded in producing as much change as would have corresponded to well-marked varieties, if both had been exposed to the light. When, however, careful qualitative and comparative quantitative analysis are compared, which appears to me to be the only correct way of studying the subject, it becomes quite apparent that there is a very interesting connection between the distribution of the fundamental colouring matters and the general organisation of plants. In proceeding from the lowest to the highest classes there is an unmistakable advancement from a type corresponding in certain particulars with that of some of the lowest animals to that of the highest classes of plants, as though certain colouring matters were more characteristic of, and perhaps indeed essential for, the healthy growth of the most perfect and specialised types of vegetable life. There are also remarkable examples of the changes in the colouring of particular plants, according as they grow in strong light or in such very shady situations that the vitality is very low, and on comparing the qualitative and quantitative differences it may be seen that in several important particulars they correspond with the differences met with in higher or lower classes, the effect of the comparative absence of light being to lower, and the effect of the presence of extra light being to raise the type. The most striking instance of this so far met with is in the case of *Oscillatoriæ;* for when they grow where the light is so feeble that they can only just keep alive, the type of their colouring approximates to that of olive *Algæ*, whereas, when they grow exposed to much air and light, the type approximates very closely to that of such lichens as *Peltigera canina*.

In order to show the kind of evidence on which such conclusions are based, and also to illustrate what I mean by

comparative quantitative analyses, I subjoin the following table. It would have been of very little use to have compared equal weights of different plants, since the amount of endochrome is so very different, and it is the variation in its composition that is of chief interest. It was therefore necessary to assume some one constituent equal in all cases, and the only one suitable for this purpose was the blue chlorophyll. This, then, was taken at 100 in all the specimens, and the relative amount of the other constituents calculated accordingly, 100 being taken as the maximum quantity for each kind of colouring matter. By adopting this plan, of course the amount of some of the constituents is made to appear as though it were increased by greater exposure to light, in the same order as they are less and less decomposed by its action; but in reality the amount is diminished in the opposite order, as would be made apparent by assuming as equal the constituent least changed by light, or by taking an equal weight of the different specimens of each separate kind of plant.

	Blue Chlorophyll.	Chlorofucine.	Phyco-xanthine.	Orange Xanthophyll.	Fucoxanthine.	Lichno-xanthine.
Fucus grown in the shade	100	90	0	3	77	11
" " sun	100	100	0	3½	100	14
Oscillatoriæ under water in a very shady place	100	13	0	1	51	6
Oscillatoriæ in more light	100	19	36	3	55	10
Oscillatoriæ in and on water, exposed to the sun	100	trace	67	25	11	9
Oscillatoriæ on a damp wall quite open to the sun and air..	100	trace	100	77	25	23
Peltigera in a medium amount of light.. ..	100	trace	27	32	0	32
Peltigera exposed to much sun	100	trace	54	100	0	100

On comparing the relative quantities of the different substances, it will be seen that, as before named, very great changes are due to the difference in the amount of light, some of which may be referred to its direct decomposing action, and others to its indirect influence on the constructive energy of the plant. The most important general fact, however, is that, when the *Oscillatoriæ* grow in a very feeble light, the phycoxanthine and orange xanthophyll almost or quite disappear, whilst the amount of fucoxanthine increases, so that the general relation of the colouring matters approaches that of the *Fucus;* whereas,

when they grow in bright light, the amount of fucoxanthine is much decreased, and that of the lichnoxanthine considerably increased, whilst the phycoxanthine and orange xanthophyll are developed to a remarkable extent, so that the general type approaches that of such lichens as *Peltigera*.

There is also a well-marked tendency to approximate to a lower type of colouring in the case of those permanent varieties of plants which have very yellow leaves, on account of the amount of chlorophyll being abnormally small. The green leaves of the higher classes of plants contain two different kinds of chlorophyll, which give quite different spectra, and differ in various other important particulars. These I have named *blue chlorophyll* and *yellow chlorophyll*, from the difference in their general colour. Now the small quantity of chlorophyll which exists in the above-named leaves contains only about one-third the relative amount of the yellow chlorophyll, which corresponds to what is met with in leaves abnormally yellow from being grown almost in the dark, as if both were due to low constructive energy, one natural to the variety, and the other produced artificially. Both differ greatly from green leaves which have turned yellow by fading, for these contain double the normal relative amount of yellow chlorophyll, which is not so readily formed, but, when it has been formed, is not so readily decomposed as blue chorophyll. This reduction in the relative quantity of yellow chlorophyll causes leaves abnormally yellow, owing to low constructive energy, to approach to the type of red *Algæ*, in which this energy is so low that blue chlorophyll is developed alone, and yellow chlorophyll is quite absent. If further research should prove the existence of other examples of this kind of fact, and establish it as a general law that when the healthy development of the higher classes of plants is arrested the type of colouring approaches to that of lower classes, it will be very instructive in connection with the theory of evolution, and analogous to what is so common in the general structure of animals, in which when their development is arrested it often approximates more or less to that of those of lower organisation. It would also indicate that in some way or other the constructive energy of the lowest classes of plants is lower in the scale than that of the highest, but it does not follow that plants with this higher type of constructive energy could live in more variable and adverse conditions than those with a lower type.

It would be impossible to select a better example of the manner in which different groups of plants are related, and

also distinguished by their colouring matters, than the leading divisions of marine *Algæ*, viz., the olive, the red, and the green. These very seldom contain accidental colouring matters, and the result is that the general colour is such a good indication of the kind and relative proportion of the fundamental colouring matters that it has been generally recognised as a valuable means for arranging *Algæ* into the above-named three divisions, though the true relations and differences were unknown. The total number of the fundamental substances is about twelve, and for a description of their distinguishing characters I refer to my paper, recently published in the "Proceedings of the Royal Society" (vol. xxi., p. 442).

Now these various substances are distributed very differently through the different groups, so as to connect and yet distinguish them in a very definite manner. I have not been able to make accurate quantitative analyses, and, besides that, there is a considerable variation in the relative quantity of some of the constituents of the red group, and therefore it is only possible to give a general tabular view, by expressing the relative amount of the various substances by means of the following signs :—

A relatively large quantity	❊
,, ,, moderate ,, ...	+
,, ,, small ,, ...	•

	Olive Group.	Red Group.	Green Group.
Blue chlorophyll	+	+	❊
Yellow chlorophyll			+
Chlorofucine	+	•	
Orange xanthophyll	+	+	+
Xanthophyll		+	+
Yellow xanthophyll			+
Fucoxanthine	❊	•	
Lichnoxanthines		•	+
Phycocyan		+	
Pink phycocyan		❊	
Red phycoerythrine		❊	

On inspecting this table it will be perceived that the olive *Algæ* are characterised by the large amount of chlorofucine and fucoxanthine, and by the total absence of yellow chlorophyll, of xanthophyll, and of yellow xanthophyll. The red *Algæ* are especially distinguished by the colouring-matters of the phycocyan and phycoerythrine groups, but also differ from the olive in containing xanthophyll and only a little chlorofucine and fucoxanthine. The green *Algæ* are charac-

terised by the presence of yellow chlorophyll and yellow xanthophyll, as well as by the absence of chlorofucine, fucoxanthine, and the substances specially characteristic of the red group. Blue chlorophyll and orange xanthophyll are common to all, and it is probable that no class of plants except fungi is ever quite free from both of them. It will also be seen that the red group is intermediate between the olive and the green, and independent of the colouring matters specially characteristic of the red, it differs from each of the other groups far less than they do from one another, and, besides this, there are still closer connecting links, not shown in the table.

My endeavour has been to extend such a method of comparison to all the leading classes of plants and some of the lower classes of animals, and to ascertain the order in which they should be arranged, so as in like manner to have the most gradual and unbroken passage from one to the other. Comparing these various groups of *Algæ* with other classes of plants, and with such low classes of animals as *Actiniæ*, I found that the whole of the colouring matters present in the green *Algæ* are those most characteristic of all the higher plants, the only difference being that in certain circumstances these latter contain in addition various more or less accidental substances belonging to the erythrophyll and chrysotannin groups, which to some extent appear to be characteristic of particular classes. As far as colouring is concerned, the green *Algæ* are therefore perfectly typical plants. On the contrary, the olive *Algæ* differ from them in a very marked manner. They contain no yellow chlorophyll, or either of the two kinds of xanthophyll, all three so very characteristic of the most perfect plants, but contain chlorofucine and fucoxanthine, both of which are found in certain species of *Actiniæ*, like *Anthea cereus*, var. *smaragdina*. The presence of such colouring matters, therefore, connects them with some of the lowest classes of animals, in the same manner as the presence of chlorophyll connects such animals with plants.

I have extended this method of comparison to many other cases, but much remains to be learned before the exact connexion of all the leading groups of plants can be looked upon as established in a satisfactory manner, and I have hitherto been unable to obtain suitable material for thoroughly investigating the relation between the lowest classes of plants and animals. Though I look upon my present results only as a beginning of the subject, it may, perhaps, be well to explain what is the general bearing of

the facts so far determined, and to give a tabular view of the manner in which some of the different classes of plants should be arranged, so as to be in the order of the most simple continuity. This table, of course, refers only to the chromatological characters, and since we could scarcely expect them to follow the same order as the structure, we cannot be surprised to find that the order of arrangement is not exactly the same as that so commonly adopted, and yet the general agreement is sufficient to show that a similar great principle is common to both.

Actiniæ.
Anthea cereus, var. *smaragdina.*
Olive group of Algæ.

Red Algæ.	*Oscillatoriæ.*
Porphyra.	*Peltigera.*
Green Algæ.	*Lichens.*

Higher Cryptogamia.
Highest classes of plants.

It was some time before I could understand how fungi should be placed in this arrangement, for they could not be inserted anywhere in the direct series. At length I found that on the whole their most prevalent colouring matters correspond with those characteristic of the fructification of lichens, and that fungi, therefore, bear much the same relation to lichens that the flowers of a leafless parasitic plant bear to the foliage of the highest class of plants. This conclusion, derived from the study of the colouring matters, agrees so well with what has been deduced from other quite independent data, connected entirely with structure, that it must be looked upon as additional evidence of an important relation between the general organisation of plants and their coloured constituents; which unites with other facts in showing that they are not mere chemical products, formed under such conditions as can be imitated artificially, but in some way or other depend on structure, or on forces connected with it in living organisms.

My attention has lately been directed to the study of the changes which occur during the growth of the various organs of plants. For example, I have compared the constitution of the endochrome of the petals, when in a rudimentary state, with that of leaves and fully developed flowers. Even so far the results are of much interest. The endochrome of the rudimentary petals approximates in character to that of leaves; and, during their development, this leaf-like character is gradually lost, and often new colouring matters are formed. Differently coloured varieties are often simply cases in which this development is arrested, so that some, when fully grown,

correspond to others in a more rudimentary state; and if the development be arrested by unfavourable conditions, artificially produced, this rudimentary character of colouring is retained when the petals are fully grown. One of the most remarkable facts is, that in some cases, if we slowly oxidise the mixed colouring matters dissolved out from a flower grown in the light, by adding a little turpentine, or by exposing the solution to the sun, the relative proportion of the different substances is changed, so as to closely correspond to that met with in the same kind of flower grown nearly in the dark. Exposure to light thus produces the same effect on the dead colouring matters that absence of light produces in the living plant, which seems to show, that, when the constructive energy is weak, those substances which are most easily decomposed are not sufficiently protected from decomposition. The study of such changes during the growth of other parts of plants cannot, I think, fail to throw much light on several interesting questions.

Such, then, is a brief account of some of the leading features of what appears to me to be a very promising branch of research. There are many questions connected with it that I have alluded to in the most incidental manner or not mentioned at all. The study of the action of light on the various coloured substances when in different conditions, and dissolved in different liquids, either when alone or mixed with others, is of itself a wide field for inquiry, well worthy of attention, since it may serve to explain the manner in which the energy of the sun's rays becomes stored up in the various compounds formed by plants. I have studied this action very carefully, and though I have been able to detect what appear to be general laws of much interest in connection with optics and chemistry, very much still remains to be learned. The chemical relations of the various colouring matters require much further investigation, in order to ascertain whether and in what circumstances one may be artificially or naturally changed into another, which is especially interesting in connexion with the colour of the petals of flowers. It is also very desirable that the connexion between the decomposition of carbonic acid and the changes that take place in the colouring matters when exposed to the sun, should be more fully examined, since, when separated from the plants, their decomposition by light seems to be a process of oxidisation, which, of course, is the reverse of what occurs when living plants absorb carbonic acid and give off oxygen. Perhaps it is only those portions of the endochrome which in some way or other have lost their normal power that are thus

destroyed, so as to make way for the younger and more active. It will also be requisite to still further study the variations in the spectra of the different colouring matters which depend upon the conditions in which they occur, since in some cases it is thus possible to ascertain whether they naturally exist in a free state or are combined with oily or waxy substances. This makes such a remarkable difference in the spectra of some yellow flowers, that for a long time I thought that the colouring matters were entirely different, but I have now found that when oil penetrates to the endochrome, so as to combine with the colouring matter, the spectrum is changed to exactly the same as that met with in other cases which are not thus changed by the addition of oil, as though sufficient had been naturally present. In these experiments, the petals must be well crushed, so as to burst open the cells, and then dried, or else the oil will not penetrate to the endochrome. By carefully examining the position of the absorption-bands, we may not only determine such facts as these in the case of a colouring matter insoluble in water, but when they are soluble we may sometimes prove that they are not dissolved in the aqueous juices in the living plant, but do become dissolved when decomposition takes place, as though perhaps originally enclosed in minute cells, and set free when the cell-walls decay. Independent of these various general questions, the study of all the leading classes of plants, and of a number of the lower classes of animals, is necessarily a very extensive subject that can only be worked out by degrees, on account of the difficulty of procuring the requisite materials, in a fresh state, at the proper season of the year; and it is made still more extensive by the necessity of examining the same plants at different periods of their growth, and when grown under different natural and artificial conditions. On the whole, then, it will be seen that comparative vegetable chromatology, in its full extent, including everything requisite for its successful investigation, is a very wide and almost new branch of science; and, though I have accumulated a large amount of facts, I cannot but feel that it is only in its infancy. Still, however, the brief account of it which I have now given will, I trust, suffice to show that even already it has thrown a new light on a number of facts, and that a further and more complete study will probably enable us to examine some of the most important questions connected with vegetable physiology and the evolution of plants, from a new and independent point of view.

IV. PEAT.

By Frederick Charles Danvers, Assoc. Inst. C.E.

As all questions connected with fuel are, at the present day, of primary importance, it is proposed, in the present article, to investigate the subject of peat, and the treatment it undergoes in order to fit it for all the different purposes, domestic and industrial, to which it has been, and is now, in some part, applied. Before, however, dealing with the various means by which it is prepared, it will be advisable to give some few details of its nature and peculiarities, as well as such scientific and statistical accounts of its available quantity and commercial value, as may tend to demonstrate more fully the vast mine of wealth lying hidden beneath the surface of bogs, and which is too often half neglected even where it is not wholly ignored and unutilised.

Notwithstanding the inferior calorific value of peat, which we shall further allude to presently, it has several undoubted advantages over coal, of which we may specially notice that it is more easily worked, and that without the necessity of any large expenditure on plant and machinery, such as is required for coal mines; the working of a peat bog is unattended with those dangers to life and limb which are so characteristic of coal mines; peat is reproductive, and can be cultivated, whereas the supply of coal existing in any country is incapable of being maintained, and must in course of time become exhausted. Peat can consequently be worked more cheaply and economically than coal. On the other hand, as we have already stated, the calorific value of peat is inferior to that of coal; its specific gravity being lighter, it is also more bulky, and consequently more costly, bulk for bulk, in carriage. Peat produces several valuable products on distillation, to which we shall refer more particularly as we proceed; it also produces a very pure charcoal which is highly esteemed in many manufactures. The best mode of carbonising peat for charcoal has been the subject of numerous experiments, but space will not admit of our entering upon this part of the question in the present article.

It will thus be seen that, although peat is undoubtedly inferior to coal as a fuel, it yet possesses many valuable properties and advantages which render it well suited as a substitute for coal, especially in localities in the near

proximity to peat bogs, and where the carriage of that fuel would, therefore, not bear too high a proportion to its economic value.

According to Berthier and Regnault, peat gives from 28 to 30 per cent in weight of coke; lignite, 40 to 50 per cent; bituminous coal, 60 to 80 per cent; anthracite, 80 to 90 per cent; and graphite, 92 to 94 per cent of its weight. From a view of this table, it appears not at all improbable that we have in the above-mentioned substances a regular gradation of the action of nature in the production of pure carbon from vegetable substances. If we go a little further back, and take vegetable matter in the form of wood, we find that the amount of charcoal obtained from different kinds of wood varies from 16·4 per cent from Scottish pine to 26·0 per cent from lignum vitæ; the proportion of charcoal from the latter being therefore nearly equal to that from peat.

The following table of calorific values of combustibles, compiled chiefly upon the authority of experiments made by M. Péclet, is taken from M. Bosc's valuable work, "Traite Complet de la Tourbe," a book which affords more valuable information on the subject of peat generally than any book hitherto published:—

Combustibles.	Calorific value of one kilogramme.	
	Dried.	Containing 25 per cent of water.
	Calorific units.	Calorific units.
Wood dried at 100°	3600	2750
		With 10 per cent of water.
Anthracite	8000	7150
Pure carbon	7800	7000
Wood charcoal	7300	6500
Coke from moulded peat	7400	6500
Coal (first quality)	6000	5350
Ditto (second quality)	5500	4850
Coke from coal	6500	5800
Ditto from ordinary peat	5500	4900
Purified moulded peat	4500	3900
Ordinary peat	3200	2800

Experiments undertaken by the Chemin de Fer de l'Est, with stationary engines and locomotives, at their depôt at Epinal, both with coal and with peat obtained from the manufactory of M. Laroche and Co., at Sautaures, in the Vosges, gave the following results:—

Stationary Engine.—Peat consumed as fuel from the 1st

to the 15th of August, 4500 kilogrammes (about 10 cubic metres). The machine was kept running for six hours per diem, consuming 50 kilogrammes per hour. With coal fuel the same engine running six hours per diem consumed 30 kilogrammes per hour. The peat gave a lively and clear flame, equal to that of coal, and it emitted no bad smell; the 4500 kilogrammes gave 7 kilogrammes of cinder and very little ash.

Locomotive Engine.—The same peat as fuel in locomotives, from the 15th to the 30th November, was tested to the amount of 4500 kilogrammes, or 10 cubic metres. Engines in reserve consumed 30 kilogrammes per hour while standing still, and 20 kilogrammes per kilometre when drawing goods trains. Similar engines burning coal fuel consumed 20 kilogrammes per hour, and 15 kilogrammes per kilometre, under similar circumstances respectively.

From these experiments it would appear that peat fuel is equal to two-thirds of the value of coal, and this is the standard of value which is most generally given to it; but much necessarily depends upon the purity of the peat itself, and upon the method in which it has been dried and prepared.

On the Bavarian State railways, it is stated that one cubic foot of ordinary air-dried turf, of rather a light description, and weighing from 16 to 18 lbs., as used there in the locomotive, is considered to be equal in heating power to about 13 lbs. of compressed turf, or 13 lbs. of lignite, or $7\frac{1}{2}$ lbs. of coal.

Last year, owing to the exertions of Mr. Alderman Purdon, of Dublin, a Commission was organised "for the purpose of investigating, in the public interests, by personal examination in the fullest manner, such of the best modern systems of preparing improved fuel from peat as are now to be found elsewhere." The report of this Commission was presented in January last, in which, with reference to the economic value of peat as compared with coal, they remark very justly that "various estimates have been put forward of the relative heating powers of coal and turf—their comparative values as fuel—some rating peat as less than half the value of coal (weight for weight), others at two-thirds. These estimates may, in different cases, be true, for the value of peat is very variable, depending on its quality, density, and dryness; and in the combination of these, no two samples may be found identical."

Peat, as it is cut from the bog, contains from 70 to 90 per cent of water. Air-dried turf usually contains from 18 to

25 per cent of water. It does not appear to be capable of being dried in air beyond a point at which it will continue to retain about 15 per cent of water; and even when dried in a stove, it is never reduced beneath 7 or 8 per cent.

According to Dr. Ure, the calorific power of dry turf is only about half that of coal. This power is, however, immensely diminished in ordinary use by the water which is allowed to remain in its texture, and which the spongy character of its mass renders it very difficult to get rid of. Again, we find it stated, in "Tomlinson's Cyclopædia," that 7 lbs. of properly dried peat will evaporate the same quantity of water as 6 lbs. of Newcastle coal. No reliance can, however, be placed on any of these statements, so far as they concern the actual value of peat as a fuel, without more detailed information relative to the analysis of the peat used, the mode in which it was prepared, and the analysis of the coal against which it was tried. In the absence of this information, all reports on the relative values of peat and coal are unreliable, except as regards the experiments to which they relate, and worse than worthless for general purposes of comparison, as they are calculated to mislead rather than to give substantial facts that will apply in other cases and in other localities than where such results had been obtained. The fact, which is too often lost sight of by the advocates for the use of peat, is that that fuel varies in its nature and properties to a far greater extent even than coal. In speaking of the value of peat, therefore, care should be taken to define the quality of the substance referred to, in some such a manner as we now refer to coal of different kinds under the names of "anthracite," "steam," "bituminous," "cannel," &c., &c.; and until this is done, the statistics relative to peat and its performances must possess but a very questionable value. In order to show more clearly what we mean by this observation, we shall give presently some further particulars regarding the analyses of different peats, and the properties varying with its increased age, and its relative position in the peat bog. We shall thus see that great discernment is necessary for the judicious and economical working of a peat bog, as well as in its subsequent preparation as fuel.

According to Dr. Percy, the specific gravity of peat varies from 0·25 to from 0·6 to 0·9, and it varies in its contents of carbon from 32·28 in peat from Cashmere to 61·04 in peat from Kilbeggan, in Ireland. The value of peat as fuel will, of course, as a rule, vary according to the amount of carbon it contains, the peat having the highest

proportion of carbon possessing also the greatest calorific value. The use of machinery and other appliances in the preparation of peat is chiefly to increase its density and to expel as much as possible of the water which it naturally contains. Now Sir R. Kane has stated that it is very usual to find the peat of commerce containing one-fourth of its weight of water, and that when dried in the air it will contain one-tenth of its weight. Hence the necessity for resorting to artificial means for getting rid of the water, which only has the effect of detracting from the value of the fuel; and it will be our object to point out some of the numerous devices which have been proposed for this purpose, and of the results obtained from them.

Almost all authors who have written on the subject of peat—with the single exception, so far as we are aware, of Dr. Zimmermann—assert that peat is a recent formation; for there appear never to have been found in it any remains of antediluvian animals, whilst bones of the ox, and the horse, horns of the stag and of the roebuck, and tusks of the boar are not of uncommon occurrence. The nature of the soil upon which peat bogs rest is that of ordinary vegetable earth, such as any other vegetation might grow upon. Frant and Sthel accord to peat a mineral origin; but there can, we think, at the present day, be few, if any, who would agree with them in this respect. Dejan says that peat is produced by marsh reeds and other aquatic plants, the stalks of which multiply, cross one another, interlacing themselves, and thus end by forming a solid mass of vegetable fibre. Other authors attribute the formation of peat to forests which, by reason of some natural phenomenon, have become thrown down and submerged, and, by a partial decomposition of wood and leaves, have given birth to peat. Zimmermann, again, gives another account of the growth of peat, which, he says, is formed of the decomposed roots of a group of plants, called *Sphagnum*. Peat of ancient formation, however, that author considers to be principally composed of the foliage and stalks of a reed-grass, or rush, of the roots of various aquatic plants, and of some peat-turf.

That peat is of vegetable origin there can be no reasonable doubt, but it appears in many varieties, according principally to its age and the circumstances of its origin. The foregoing remarks relative to the nature and character of peat, may at first sight appear to be not relevant to the subject more especially under consideration, namely, the different methods of preparing peat for fuel and the machinery

employed therein. A little consideration will, however, prove the contrary; for it must be admitted that a correct knowledge of the nature of the material to be operated upon must necessarily precede any attempt to define the method by which it may be most advantageously prepared for its destined use. Its economical development also necessitates the consideration of questions affecting the working of peat bogs, as well as the possibility of their cultivation and extension.

Generally speaking, peat may be divided into two classes: that which contains nothing but terrestrial vegetable matter is called "bog peat," whilst that in which is found marine vegetable matter is known as "marine peat." Again, some authors recognise three classes of peat, which they distinguish as follows:—1. Bog peat; 2. Open country peat; and 3. Mountain peat. Sometimes two of these will be found in the same bog, merging, by insensible degrees, from the one to the other.

I. Bog peat is formed at the mouths of great rivers and of streams, as well as on their banks, on lakes, on ponds, and on the sea shore. The indispensable conditions for the formation of this kind of peat are a bed of water, rather shallow, and with only a moderate current, and the presence of certain vegetable matters, of a perennial growth, which, dying down each year, deposit at their roots the decaying vegetable matter, which, by constant accumulation, produces what is known as peat.

II. Messrs. Rennie, Dr. Walker, and Ch. Patin, besides others, consider that a great number of peat bogs owe their origin to a sudden destruction of forests; and this theory is strengthened by the fact that in many open country peat deposits there are found the trunks of trees embedded in the moss.

III. The mountain peat is formed chiefly by an innumerable quantity of mosses, of the genus *Sphagnum*. These mosses hold water in their stems like a sponge. They spread their roots and suckers over the moist *débris* of wood or other similar matters, and matting themselves together as they grow, form a species of felt, which, in course of time, yields the matter known as peat. One species of this plant, the *Sphagnum cuspidatum*, is so prolific that a single pod is said to contain no less than 2,800,000 seeds.

The cultivation and reproduction of peat is a subject regarding which there is now no longer any doubt. Experience of living witnesses has proved that it takes from

thirty to forty years for a peat-bed to grow to the extent of one metre in depth, or about three metres in depth of good peat for fuel is produced in the course of a century.

It must be borne in mind that it is not possible to work all peat bogs with profit, and they are therefore again subdivided into those that are workable and unworkable bogs. When a bog is of a sufficient extent, and its peat of a good quality, not only should it be worked, but it should also be cultivated, the same principles of reproduction being applicable to peat bogs as to scientifically worked forests, where a system of clearing and planting are going on continually. One advantage of peat crops is that they have not to be gathered year by year, but the longer they are left, the greater and better in quality is their yield, and they are also independent of those variations in meteorological conditions which too often lead to the deterioration of other crops. After a careful consideration of those plants which are most productive in the formation of peat, it is necessary, for the sake of obtaining peat of good quality and purity, to protect the bog from the introduction of foreign matters. As these bogs are mostly situated in valleys, surrounded, for a part, at least, of their circumference, by hills, care must be taken to prevent silt being carried over their surface by heavy rains; the best way to accomplish this being to cut a trench round that portion of the bog exposed to such incursions. Unworkable bogs are those which do not produce peat of a sufficiently good quality to make it worth while to work them for fuel; these should be drained and cultivated.

Having now briefly treated of the nature and properties of growing peat, it may be interesting, before passing on to a description of the various methods of preparing it, to give some idea of the extent to which it is known to exist in different countries, in order to show how vast an area is available for the production and growth of fuel.

In France alone the peat deposits cover an area of about 1,200,000 hectares. They are spread over 58 departments, and are found in 5140 different localities. It does not appear that there are any peat bogs in Algeria; and, indeed, it is rather the exception than the rule to find this fuel growing in tropical climates. It has been stated by some authors that a temperature below 4° centigrade is necessary for the formation of peat, but this does not appear to be fully borne out by actual experience; for, as we shall presently show, peat deposits are to be found almost all over the world, but they are unquestionably most numerous in the more humid and temperate climates. In Belgium, the principal peat

bogs are to be found in the environs of Ath and Antwerp, on the banks of the Escaut and of its tributaries, and in La Campine Belge. Peat deposits form almost the entire soil of Holland. The site of the Haarlem lake, upon which, in 1573, the Spaniards and Dutch engaged in naval combat, and which is now drained, and covered with luxuriant farms, consists entirely of a reclaimed peat bog. In Italy, one of the most remarkable bogs is that of St. Martin-Perosa, in Sardinia, which has an area of from 4000 to 5000 hectares, and a depth of from 4 to 8 metres. Piedmont also furnishes peat in some provinces; and it is stated that many of the paper and cardboard manufacturers about Turin employ 80, and sometimes as much as 90 per cent of peat in the pulp of their cardboard. Peat is also very generally found in most of the districts of Lombardy. The peat area of Denmark is estimated at about 180,000 hectares. In England but little attention has been given to peat, but the bogs in Ireland are said to cover about one-sixth of its entire area. Bogs of large extent also exist in Germany, Prussia, Russia, America, and Canada. Mouhot,* in his travels in Indo-China, &c., mentions the discovery of a peat bog in the Ko-Man Islands of Siam. In an interesting paper read before the Society of Arts, on the 27th of January, 1871, Lieutenant-Colonel Romaine Wragge produced unquestionable evidence of the existence of peat in many parts of India; and in China † it is certain that peat exists in one locality at least, and it is not improbable that, if sought for, it would be discovered in other parts, also, of that country.

Enough has now been stated on this portion of our subject to prove that peat exists almost universally, wherever circumstances favourable to its existence are to be found. The next point for consideration is the chemical constituents of peat, for upon this depends the possibility of its being converted into a serviceable fuel. Subjoined are some of the most important analyses of peat obtained from various quarters.

The specific gravity of peat varies considerably, according to the nature of the peat moss. Its density varies also with the degree of its dryness. Ordinary peat, according to M. Bosc, is about 170 kilogrammes per cubic metre, and compressed peat from 600 to 800, and sometimes 900 kilogrammes per cubic metre.

* Page 148.
† "Industries de l'Empire Chinois," par M. Paul Champion, page 10.

Dr. Percy, in his work on Metallurgy, gives the following particulars of peat from Ireland (four samples), and of peat discovered by Dr. Hugh Falconer on the banks of a lake in Cashmere :—

Locality.	Density.	Carbon.	Hydrogen.	Oxygen.	Nitrogen.	Ash.
Ireland :—						
Philipstown .	0·405	58·69	6·97	32·88	1·45	1·99
Ditto . . .	0·669	60·48	6·10	32·55	0·88	3·30
Wood of Allen	0·339	59·92	6·61	32·21	1·25	2·74
Ditto . . .	0·639	61·02	5·77	32·40	0·81	7·90
Cashmere . .	{Water. 10·40}	32·28	3·66	21·03	1·81	29·80

The following are analyses by M. Regnault :—

Locality.	Carbon.	Hydrogen.	Oxygen.	Ash.
Vulcaire, near Abbeville .	57·05	5·63	21·76	15·58
Longprès	58·09	5·95	2·13	14·61
Champfen (Vosges) . . .	57·79	6·11	20·97	15·35

We have here purposely limited ourselves to a notice of the analyses of peat of the best quality—with the exception of that from Cashmere. The latter will afford some idea of the wide range in variation of constituents which may be found in peat. We now give one or two analyses of the component parts of peat obtained by distillation :—

Locality.	Charcoal.	Tar.	Ammoniacal Liquid. and Water.	Gas.	Ash.	Loss.
Vesle (Marne).	15·45	6·80	38·90	18·60	19·25	1·00
Kœnigsbrunn .	24·40		70·60		5·00	—
				Nitrogen.	Carbonic acid.	Carb. of Hydrog.
Baviere . . .	40·25	24·50	14·10	0·27	10·80	10·18
				Gas and loss.	Cinders.	
Brunswick . .	33·60	5·00	35·00	20·00	6·40	—

The lower the stratum of the bog from which peat is extracted, the more valuable it is as a fuel. The upper surface of a bog is often styled *turf* to distinguish it from the *peat* which lies below. When peat is cut in the ordinary manner, with the view of being burnt as fuel after having merely been dried, and without any further preparation, a particular shaped spade * is used, which cuts the peat at once in

* At the Government works, at Haspelmoor, in Bavaria, much of the turf used as fuel in the locomotives of the state railways is less, on an average, than two inches in thickness; the object of this being to facilitate and expedite the process of drying. At an Agricultural Exhibition, at Munich, the attention of the Irish Commission was attracted to a double plane, intended for cutting two sods of turf at a stroke, which they considered well suited for raising thin sods.

blocks of the required size, and these are then stacked in small open heaps to dry in the air. Where there is much water in the cuttings, dredgers of various sorts are used for raising the peat. The object of the present paper, however, not being to describe antiquated processes, but rather to take some notice of the modern improvements in the treatment of peat which the recent high price of coal has called forth, we shall abstain from any further remarks upon the preparation of what is known as "ordinary" peat, that is, peat simply cut and dried in the open air, without any further attempts at artificial preparation.

To obviate the natural inconveniences arising from the use of raw, or "ordinary," peat, attempts were made in 1821 to compress it into blocks. This plan was first commenced in Germany; afterwards it was adopted in Sweden, at the iron mines of Eckman; subsequently it was introduced into France; and, lastly, it was brought to England about the year 1837. Numerous processes have been patented for the purpose of compressing peat, but they have been unattended with any satisfactory results. At first attempts were made to compress the peat when in a partially dry state, but this proved a failure, as it did not achieve the desired end. Attention was next directed to means for compressing it whilst moist, and for this purpose powerful hydraulic presses were used; but they, instead of driving out the moisture, tended to confine it within the fuel. This plan failing attempts were made to substitute chemical agencies for the expulsion of the water, and the formation of the peat into compact blocks; but, as might have been expected, such treatment would no more produce the desired results than force unscientifically applied as above referred to.

At Haspelmoor and at Kolbermoor, in Bavaria, there are works in which compression of peat by force is, however, carried out at the present day. The system, technically known as "Exter's," consists in obtaining the peat from the surface of a bog (previously well drained and levelled) in the condition of a fine mould or powder, which, when partially dried by exposure to the sun and air, is scraped together in thin layers, and removed to the place of manufacture, where, being dried more fully by artificial heat, it is subjected to powerful mechanical compression effected by steam machinery. The cost of this compressed turf for the past year was estimated at about equal to 12s. sterling per ton. This system is essentially the same as that for which works were at one time erected at Derrylea, in Ireland, where the results were commercially unsuccessful. At

Kolbermoor the bog surface is harrowed by portable steam power, and the exhaust steam from the fixed compressing engine is employed in drying the fine turf mould. These factories are occupied chiefly in producing compressed turf for locomotive purposes.

With respect to the efforts that have been made from time to time for depriving raw peat of its water by hydraulic or other mechanical pressures, it may be stated that all such attempts have been entirely unsuccessful. This is not to be wondered at when we consider the nature and properties of peat.

According to different authorities raw peat, when freshly dug, contains from 75 to 90 per cent of water, and 100 tons would therefore only produce from 10 to 25 tons of fuel—more generally the former quantity than the latter. How to get rid of this water in a cheap and expeditious manner was, therefore, the important point requiring solution.

Peat consists of a number of stems or stalks of the *Sphagnum*, or other peat plant, closely matted together with partially decomposed vegetable matter. These stems all contain capillary tubes, which hold the water with great tenacity, and from which it can only be expelled by the complete destruction of the capillary. Hence we see that the only true method of treating peat is by maceration and precipitation, for it is found that when thoroughly macerated the precipitated particles, uniting themselves by a chemical affinity, discovered by M. Chaleton, a French chemist, causes the pulp to discharge the water contained in it, and to obtain a density nearly the same as pit coal.

This method of preparing improved fuel from peat is based upon the fact that when raw peat is subjected to any treatment by which, in the wet condition, its fibrous structure is broken up so that the whole forms a homogeneous mass of pulp, it will not only dry more rapidly, but will acquire a cohesion and density in the process of drying greater than any ordinary mechanical pressure could impart to it. This quality of density gives not only an increased value to turf as fuel, but also the great advantages arising from increased facilities of transport. Turf, thus treated, on being thoroughly dried, will sometimes become reduced to one-sixth part of its original bulk or volume.

The principle of maceration in the preparation of dense turf is found in the making of "hand-turf," as practised in different parts of Ireland. The capability of hand labour for the production of large quantities of dense turf is, however, limited in practice. Dense turf is produced in large

quantities in the Netherlands in the following manner, but the product is not equal to that resulting where machinery is employed for the purpose:—Owing to the position of several peat districts in the low countries, much of the peat lies under water, and, when raised by dredging, is conveyed to working places, where it is well kneaded or trodden under foot, and at the same time freed by hand of any roots or other substances that would interfere with bringing the whole to a fairly uniform mass. The spreading ground is generally strewn with some loose, dry material, such as broken reeds, and in this way adhesion to the surface is prevented, and some opportunity is given for the escape of moisture from below. In the kneading operation short pieces of boards are attached under the feet of the workmen, and, when in this manner the mass is sufficiently levelled, it is marked out lengthwise and crosswise, and is subsequently divided by a simple tool made for the purpose, after which the usual process of drying in the open air proceeds. In Friesland and the district of the Haarlemmer the size of dense turf made in this manner averages, when dry, from 5 to 6 inches in length, and in thickness from $2\frac{1}{2}$ to 3 inches square. In other parts it is produced of reduced thickness with a view to drying in a shorter time, and made thus it has a shape not unlike flat tiles of peat, varying in size from 5 to 6 inches in length by 4 inches in width, and only $1\frac{1}{2}$ inches in thickness. The annual production of dense turf in the Netherlands supplies largely the place of coal for many industrial purposes, and presents the most extensive development of dense turf industry at present in Europe.

For the purpose of producing dense turf of a still better class than the foregoing the application of machinery is required for the complete reduction of the peat fibre. This process was, according to M. Bosc, first employed about fifteen years ago by the Comte de Lard, in the peat-bog of la Saussaye, in the department of Seine-et-Oise, near Paris. The method adopted by him may be thus briefly described:—The peat is extracted from the bog in the ordinary manner. It is then cast into a grinding machine, and ground up with an alkaline solution. The sludge passes into a large basin, and by the aid of powerful pumps it is raised into a second basin, 3 metres higher than the former one; from this it is permitted to flow into open wooden side frames standing on a bed formed of bundles of osier, straw, or grass, through which the water filters away, whilst the pulp left behind attains, at the end of a few days, such a consistency as to

allow of its being cut into blocks, and after a further period of fifteen days, the latter are sufficiently dry for stacking. A system of maceration and precipitation, accompanied by filtering, is adopted in the factory of Montanger, near Corbeil; and at a factory near Munich the maceration is followed by compression.

About six or seven years ago works and machinery were erected in the province of Drenthe, in the Netherlands, for the production of dense turf. Here the macerating mill is a vertical cylinder, about 6 feet high and 3 feet in diameter, in which a vertical iron shaft revolves. Upon this shaft several arms are fixed, which tear up the raw peat, after which the pulp is forced by a screw through a pipe at one side. This pipe terminates in a mouthpiece with three orifices, through which the disintegrated peat finally issues forth, and as it issues is cut off when about 12 or 13 inches long, and the pieces are removed for drying in the open air. The dense turf produced by this arrangement is of very good quality; the pieces when dry have an average size of 9 inches long by 2½ inches square, and each piece weighs nearly a pound and a half.

The manufacture of peat has for some years past been extensively carried on in Canada, by a process invented by Mr. James Hodges. The whole of the machinery employed in this manufacture is carried on vessels, which float in canals cut by themselves through the peat bogs. A pair of large screws, with cutting blades, are placed at the front end of the boat, and driven through gearing by an engine in the stern of the vessel. These screws cut their way through the bog, forming a channel 19 feet wide, and from 4 feet to 6 feet deep, and as the water flows in as fast as the peat is taken out, the vessel floats and moves onwards as the screws advance, generally at the rate of about 15 feet per hour. The peat is cut and driven by the screws into a well in the bow of the boat, it being first cleared of any pieces of wood, roots, or other extraneous matter. From the well the peat is lifted by an elevator, and discharged into a hopper, and thence into a part of the machinery which arrests all roots, &c., which have not been previously removed. After this it is pulped, and from the pulping machinery it passes, with a consistency of thick mortar, through a distributing shoot projecting at right angles to the vessel, whence it falls on to a space of ground, specially prepared, on either side of the canal, over which it is spread evenly to a thickness of about 9 inches. In a couple of days or so the pulp is sufficiently dry for the next operation, namely, that of cutting the peat

transversely, which is done by hand with tools specially adapted for the purpose. A few days afterwards it is in a fit condition to be cut longitudinally; the size of the slabs, or bricks, being 18 inches long by 6 inches wide. A fortnight later—if the weather be favourable—the bricks are hard enough for stacking, and after several days more they are turned and re-stacked; ultimately they are loaded upon barges on the canal, and floated down to store. The machine, which we have thus briefly described, in ten hours' working excavates and pulps sufficient peat to give 50 tons of air-dried fuel, and in doing so it makes a navigable canal 150 feet long, 19 feet wide, and 5 feet 6 inches deep. One ton of this peat fuel measures 70 cubic feet. Experiments as to its efficiency have been made upon the Grand Trunk Railway of Canada with the following results as compared with coal and wood, with an express passenger train on a run of 177 miles:—

Average mileage run with 1 ton of coal	59·91
,, ,, ,, 1 cord of wood (4000 lbs.)	40·69
,, ,, ,, 1 ton of peat fuel . .	50·50

Taking the then relative prices of the above three classes of fuel it was found that the cost for the distance of 177 miles would be as follows:—viz., coal 29·50, wood 30·87, and peat fuel 16 dols.

It will, of course, be understood that this method of manufacturing fuel can only be carried out where a sufficient supply of water exists in the peat to fill up the channel as it is formed, and to float the manufacturing vessel forward. In the absence of a sufficiency of water for the above purpose, recourse must be had to the use of stationary and fixed machinery. Of this class there exist two principal methods of treatment: the one patented by Messrs. Clayton, Son, and Howlett, and the other the invention of Mr. John Box. These two, however, differ very materially from one another, as will be seen from the following descriptions of them:—

According to Messrs. Clayton's process, the raw peat, as dug, is filled into a special arrangement of "squeezing" trucks, having perforated sides for the escape of water from the peat. A piston forced against the peat in the truck by the aid of a screw and lever effects a pressure upon the body of the peat, and during the passage from the bog to the machine the peat is thus freed of a considerable portion of the water. The rough peat is fed from the squeezing trucks into a hopper, through which it falls down a vertical chamber in which revolves a shaft, having screw blades fixed on it,

as in an ordinary pug mill. By the action of these blades the peat is broken up and forced downwards into the comminuting apparatus. The latter consists of a horizontal cylinder fitted with a central revolving shaft, upon which are fixed propelling screws, and a series of curved arms or discs, so arranged upon it, that in their whole length they form a dissected double helix, with increased spiral. Along the bottom of this cylinder, and projecting upwards towards the shaft, are arranged cutting blades of hardened steel, between which the discs pass in their revolution. The general arrangement of this part of the apparatus is very similar to that of a huge sausage-making machine.

The peat thus fed into the cylinder is carried forward by the discs, each revolution bringing the peat against the cutters, and thereby effecting a complete mastication of its fibrous tissues and cellular structure, the roots and other undecomposed portions being reduced to a state of fine pulp, and the whole mass is brought into a uniform condition. The end from which the pulped peat issues is fitted with a number of moulding orifices through which it is forced. These may be of any desired shape and number according to circumstances. Beneath the chamber upon which the moulding orifice is fixed is a roller table, on which the trays for receiving the moulded peat are placed in succession by a bog, so that they run in a continuous series underneath the moulding orifices and receive the peat issuing from them. As the front end of each tray comes up the workman severs the streams of moulded peat by means of a sliding cutter, which severs each bar into pieces 5 inches long. The trays, thus loaded, are lifted on to racks, where they remain for about three days, until the peat will bear handling, when they are placed upon open shelves for final drying. It is stated that peat of a fibrous nature when treated by this machinery becomes compact and hard, and assumes a specific gravity of from 1·05 to 1·10, whilst black decomposed bog condenses to about 1·20.

In conclusion we have to make a few observations upon Mr. Box's method of treating peat in order to produce it in a dense form; but first we must refer to a few principles laid down by that gentlemen to be observed on this subject, as, so far as we are aware, they have not previously been insisted upon. With regard to the initiatory pulping of the peat, it must be observed that this process may be carried to too great an extent, as extreme fineness of maceration entirely defeats the object in view. Mr. Box has remarked on this subject, in a letter to the *Freeman's Journal*

of the 24th of January last, that he had seen mantel-shelves made of peat finely passed through sieves, and that it was as little subject to fire as an ordinary piece of stone. It must be remarked also that dense peat ripens by age, and acquires a greater value in proportion to the length of time it has been made; thus, in May, 1872, advertisements in the journals of the Department of the Somme, in France, quoted the price of peat fuel of 1871 at 16s. 4d. per ton, whilst that of 1870 was 18s. per ton. Peat, if properly prepared, may be used after about two to three months' drying, but it is not then hard enough to bear the weight of iron ore in a blast-furnace, nor would it resist the blast for a sufficiently prolonged period of time.

The chief peculiarity in Box's process is, that he employs a Carr's disintegrator in preference to any other kind of mill for pulping the peat; and this he has adapted to the tearing up and disintegration of raw peat mixed with water, it being found that it would not disintegrate the peat unless it was worked in water, and this is the principal change which he has made in Mr. Carr's mill.

With this machine the raw peat is torn up and divided as it comes from the peat bog, the supply of water to it being so regulated as to produce a pulp of a certain and equal consistency. The raw peat is supplied to the side of the opening of the mill by a hopper or funnel of a peculiar form; it is allowed to escape from the mill with considerable velocity, and falls a height of 4 or 5 feet on to a sieve of considerable dimensions, and rushing through it leaves all undecayed vegetable remains on its surface, from whence they are roughly raked by a man placed at its side. The pulp runs into reservoirs, which are merely spaces of ground, surrounded by planks, about 60 feet long by 40 feet wide; the bottom of these reservoirs is specially prepared, and rendered porous by under draining. The peat pulp is run into these reservoirs to a depth of about 9 inches, and within twelve hours it will often be found to have shrunk to 5 inches in thickness, and to have assumed about a consistency of prepared brick clay; at this moment a stamper, worked by a man, cuts the solidified pulp into pieces 9 inches by 4 inches. These pieces shrink from one another, and about the third day they are sufficiently dry to be handled. They are then carried away in specially constructed wheelbarrows, and placed on shelves formed of laths in small sheds or frames, some 400 to 500 feet in length, 6 feet high, and 4 feet deep, where they are left to harden. This hardening, in summer

time, takes about two months, and the peat blocks are then ready for use.

From the particulars above given, it will readily be assented that, in the application of the principle of macerating the raw peat, and in the air-drying process, will be found the only reasonable system of producing dense turf upon a commercial basis: and this is the conclusion to which Mr. Purdon's Committee in Dublin arrived. The simple air-drying of raw peat, as it is cut in blocks, with the slane from the peat bed, produces but a light friable species of fuel, and one which is inconvenient for transport, and subject to considerable waste in handling. The more fibrous the peat is, the more subject is it to both these inconveniences. The maceration of the peat into a pulp is, in effect, the hastening of the work of Nature; and its subsequent precipitation causes the production of a result in a few days equal in effect to what would have taken probably centuries in the ordinary course of events.

The question of cost of producing dense peat by maceration and precipitation has purposely not been treated of in the present article, as it has not yet been carried out to a sufficient extent in this country to enable reliable data to be obtained; besides which, many considerations have to be taken into account which differ largely in various districts, such as the price of labour, local peculiarities, the distance of the bog from the manufactory, and other points which will readily suggest themselves, so that they need not be further dwelt on here. As some guide to the cost, however, it may be as well to state, that at Brandenberg the total cost of labour in the production of dense turf was estimated by Mr. Purdon's Committee at 6s. 6d. per ton; at Prince Schwartzenberg's works near Gratzen, in Bohemia, the cost of manufacture is set down at 6s. 9d. per ton; Hodge's Canadian Peat Company produce peat at 6s. 6d.; the Boston Peat Company at 8s.; at Haspelmoor, in Bavaria, it costs 12s.; Messrs. Clayton, Son, and Howlett estimate the cost by their process at from 3s. 6d. to 5s.; whilst Mr. Box believes, that, by his process, the expense of manufacturing peat will be only 2s. per ton.

V. CHANGES IN THE MOON'S SURFACE, WITH SPECIAL REFERENCE TO SUPPOSED CHANGES IN LINNÉ AND PLATO.

By Richard A. Proctor, B.A. Cambridge, Hon. Sec. R.A.S. Author of "The Sun," "The Moon," "Saturn," &c.

THE study of our moon by astronomers has had for its main purpose the recognition of change, the effects of processes taking place on the moon resembling in some degree those with which we are familiar on earth, and especially of effects due to the existence of life, animal or vegetable, upon the surface of our satellite. It is impossible to avoid the recognition of this fact in the work of even the most systematic and rigidly scientific lunarian astronomers. The charting of the moon would certainly not have been prosecuted with the care and energy actually shown by such workers as Cassini, Schröter, Beer and Mädler, Lohrman, and Schmidt, but for the fact that such researches afford promise of an answer to the question, whether the moon is on the one hand a dead and arid waste, without any signs of motion or of change, or on the other hand a scene where systematic processes of change are taking place, which may or may not be interpretable by us, but the mere occurrence of which would suggest that the moon is the abode of some forms of life.

I propose now to enter into the discussion of the question here indicated, regarding it in the light afforded by recent researches chiefly, but not omitting the consideration of antecedent probabilities on the one hand, or of theoretical inferences on the other. For I hold that, in scientific investigation, a mere array of facts is of little force; it is from facts viewed in their relation to known physical conditions that we can alone hope for useful additions to our knowledge. And we must in a special manner avoid the common mistake of too rigidly directing our attention to the particular subject we are upon, instead of combining a careful scrutiny of that subject with the search for information and analogies derivable from other subjects, and even from subjects which at a first view may appear little connected with the one in hand. This, at least, is the course which experience suggests as the most effective, the whole history of science showing the uselessness of the mere accumulation of facts,

and the importance of the comparison of the discovered facts of one subject with the recognised truths of other and especially of more advanced subjects.

One other point is to be noticed before we proceed. The enquirer into such a subject as that we are upon, whether he endeavours to advance by means of his own personal observations directly, or by the analysis and comparison of observations made by himself and others, must not act upon any preconceived ideas as to the result of his work. In particular, he must not consider that success depends on the recognition of signs of lunar activity rather than on the acquiring of evidence pointing the reverse way. He must in fact proceed quite independently, or his labours will be worse than useless; they will be self-deceptive, or at least they will have a tendency to be so. It has been unfortunate that some of our most earnest lunarians have adopted apparently a different view, and would seem to consider that, unless they establish the occurrence of change, they have done nothing. On the contrary, if, by the rigid scrutiny of some particular part of the moon, any observer should succeed in demonstrating that there at least there has been no change, or none that can be recognised, his work is as important in its scientific aspect as though he had demonstrated that some remarkable change had taken place, though no doubt far less interesting to the general public.

The first astronomer to take up the special form of lunar research we are here considering, with direct reference to the condition of the moon as a probable abode of life, was Schröter. Fontenelle had earlier discussed the question, and had spoken of the downfall of a lunar peak, which had probably never had any existence but in his own lively imagination. Huyghens, in his "Cosmotheros," takes the habitability of the moon for granted, and speculates fancifully on the nature of the lunar inhabitants; but Schröter endeavoured observationally to establish the fact. He conceived that he had demonstrated the existence of a lunar atmosphere of appreciable extent. He supposed that he had discovered a great lunar city, north of Maurius; besides lunar canals and roads in other regions. He described the formation of a new mountain on the Mare Crisium, and its eventual disappearance; and he asserted that another had come into existence in Helicon*, and still remained, where previously there had been no mountain.

* The mountain in question disappears when the moon is full, at least for ordinary telescopes; and it was doubtless in this way as Webb suggests, that Hevelius and Riccioli overlooked it.

Gruithuisen went further even than Schröter had done; but his chief reliance was placed, not upon changes in the moon, but on the existence of certain regular formations. "He collected with the utmost diligence," says Crampton, in his interesting little treatise on the Moon, "all the objects which he beheld on the lunar surface having the appearance of regularity, such as would be shown in the works of man here;" and, strange to say, his search, with the aid of a somewhat wild imagination to help it, was by no means unsuccessful, at least as far as "finding such objects" was concerned. Yet objects of the kind appear regular only when examined with low telescopic power. In instruments of considerable size they show manifest signs of being simply natural formations.

However, it is not my present purpose to consider those features of the moon which present an aspect suggestive of artificial construction, but to deal specially with the instances or supposed instances of change.

No one can study the moon for any considerable time without being led to the conclusion that her general aspect is unvarying, save, of course, as respects the changing amount of illumination as the lunar month proceeds. It becomes clearer and clearer, the longer the study of the moon is continued, that it is by the careful scrutiny of small portions of the moon's surface that any signs of change are to be recognised.

But this general constancy is in itself a most important point in our subject. For supposing we should recognise signs of change in any small portion of the lunar surface, we shall have to enquire into the probable cause of such change; and in making such an enquiry, it will be most important to have determined beforehand whether any atmosphere surrounds the moon's globe, and if so, what is its probable nature and extent. I do not propose to discuss this point at any length here, because it is one which has been already discussed in full elsewhere; but I would invite the student of selenography to notice that all the direct evidence tends to show that there is scarcely any appreciable atmosphere round the moon, *not* in quantity (the absolute quantity may be and probably is very considerable), but as respects actual density at the mean level of the moon's surface. Now, in the enquiry into changes taking place on the moon as the result of mechanical processes, whether from the contraction and expansion of the surface, or from sublunarian forces which may still be at work, we are not compelled to consider the

density of any possible lunar atmosphere. But if we extend our theories of lunar activity, so as to include changes resembling those due to terrestrial vegetation, or again to those which living creatures might produce by their works, we are bound to take some thought of this relation. We must, in fact, consider how far it is probable that any form of vegetation, or any kind of life can exist, where the atmospheric density is as small, let us say, as in the so-called vacuum produced by one of our most perfect air-pumps. Now it appears unsafe to argue that no kind of life, animal or vegetable, can exist in such an atmosphere merely because our experience has not made us acquainted with any. But, on the other hand, it must be remembered as we proceed, that whatever degree of difficulty there may be in admitting the existence of vegetable or animal life under such conditions, is *opposed* to the occurrence of lunar changes explicable as due to such forms of life. The whole question being one of probabilities, we must not overlook this antecedent improbability.

At the same time, life exists under such varied conditions on our own earth, that it is impossible to assert that, where there is certainly very little air, and as certainly very little, if any, moisture, life cannot exist. Let us admit the possibility, and let us further admit that the strange vicissitudes to which living creatures on the moon would be exposed during the lunar day and night, are not necessarily fatal to the hypothesis that life exists on the moon.

We have, then, two forms of change to enquire into—those due to mechanical, chemical, and other like processes, and those due to the existence of life upon the moon.

But at the outset of the enquiry, we must take into consideration a circumstance which is very frequently overlooked in dealing with this subject. In all terrestrial comparisons to determine processes of change, the observer or experimenter is careful always to keep the circumstances unchanged under which the object of research is examined. If he desired to ascertain whether some distant and (let us say) inaccessible surface underwent changes, he would, to speak plainly, be careful to look at that surface in the same way throughout his experiments, and also to select occasions when the atmosphere was in some given condition.

Now, first, the conditions under which any lunar object is observed necessarily change with the progress of the lunar day. As the sun gradually rises higher and higher above the horizon of any lunar place, the shadows not only decrease in length, but shift in direction; and as the sun passes

down again towards the horizon, the shadows, though they increase again in length, are yet thrown in quite a different direction from that in which they fell in the earlier part of the day. The effect of such changes will depend partly on the nature of the surface; but all parts of the moon's surface where one would look for changes due to volcanic action, or to the effects of expansion and contraction, would be certainly very much affected by changes of illumination. Thus it is found that the whole aspect of a lunar region at morning time differs from its noon aspect, and its noon aspect again from the aspect it presents when its evening is in progress. We can take the diurnal changes into account in successive lunations, because (weather permitting) we can observe any given lunar region repeatedly at about the same hour of the lunar day. But we cannot do this with perfect exactness; for the lunar day, that is the lunation, is not commensurable with our day. Since one lunation in fact contains approximately 29·53 of our mean days, we see that if any lunar feature is observed in a given part of our sky, at a given lunar hour in one lunation, then, in the next lunation, that part of the lunar day will correspond to a time when the moon is nearly 12 hours of diurnal rotation from that part of the sky. For instance, if true full moon occurs at midnight in one lunation, so that a place on the moon's central meridian is observed at its noon and at our midnight, then, in the next lunation, the noon of that place will occur nearly at our mid-day, and the moon was on the meridian about half a day of our time before, or will be on the meridian about half a day of our time after the time of true moon for places on the central meridian of the moon; in half a day of our time a place on the moon undergoes a considerable change of illumination. Since two lunations amount to 59·06 days, that is to 59 days and nearly an hour and a half, we see, that in the next lunation but one, there will be a much smaller difference of illumination if any lunar region is observed at almost the same hour of terrestrial time; for an hour and a half of our time corresponds to only about three minutes of lunar time,* and as we know the sun's

* Since the lunar day contains 29·53 of our days, it follows that the lunar hour, or the 24th part of the day, corresponds to 1·23 terrestrial days, or 29·53 terrestrial hours. Again, one terrestrial day corresponds to $1 \div 29{\cdot}53$ of a lunar day, or to rather less than 48m. 46s. of lunar time supposed to be divided as ours (that is, the day into 24 equal parts, to be called lunar hours, the hour into 60 minutes, and the minute into 60 seconds). These two relations are sufficient for the ready conversion of terrestrial into lunar time, and *vice versa*.

position does not change much in three minutes. But then a great change will have taken place in the moon's diurnal course, simply because the moon's position with respect to the equator, at any given phase, varies as the sun's does (sometimes more, sometimes less, according to the position of the nodes of the moon's orbit, but always to a considerable degree), since the inclination of the moon's orbit to the equator is never less than 18°; accordingly a long time elapses before there is a close approach to identity in the lunar and terrestrial conditions under which a lunar region is observed.

And it is to be noted that, when so far as the moon's motion in her orbit is concerned there would otherwise be a close approach to identity in the conditions, the continual change in the inclination of the orbit causes a marked difference in the elevation at which the moon is seen above the horizon.

If we add to these considerations the fact that the moon has seasons, though they are not very marked, and that the sun's elevation at lunar noon thus varies through an arc of about 3°, we see that a very long interval must elapse before there is any very near approach to the conditions, lunar as well as terrestrial, under which any lunar region is observed.

As yet we have taken no account whatever of the lunar librations. These occasion a distinct class of differences. The varying solar elevation affects the actual aspect of any lunar region as it would be seen from one and the same standpoint; and varying lunar elevations, by causing the moon to be observed under different conditions of terrestrial atmosphere, must manifestly produce varying effects. But the lunar librations correspond to an actual change of place on the observer's part.

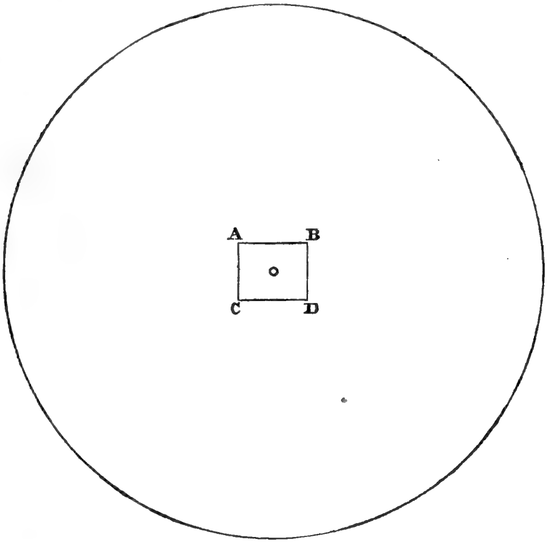
This would not be the place to give a full account of the lunar librations. In fact, the subject would require much more space than is here available. But there are certain considerations which bear in a very important manner on the question of change in the lunar surface.

Let it be noticed, that the point which is at the centre of the moon's disc when there is no libration is carried by the librations so as to occupy in turn every part of a lunar area appreciably rectangular, some $15\frac{1}{2}°$ wide in lunar longitude and some $13\frac{1}{2}°$ wide in lunar latitude. Thus the lunar region occupied by this point is viewed in every direction corresponding to these limits. We see it square when it is in its mean position, we see it tilted $7\frac{3}{4}°$ on either side of its mean position in longitude, and $6\frac{3}{4}°$ on either side of this

position in latitude, and in every possible mean position as well as with every possible combination of tilt in longitude and latitude. In fine, if O (Fig. 1) be its mean and central position, then this point occupies in turn (and in the course of time) every part of the area A B C D.

Now this has only been stated to show the actual librational sway of the moon, not to indicate the importance of the effects due to such libration. For it is manifest that the region about O cannot be very much affected in appearance by being shifted even to the point A, or to B, or to C, or to D, that is, to its maximum amount. If we were looking at the summit and slopes of any hills or

FIG. 1.



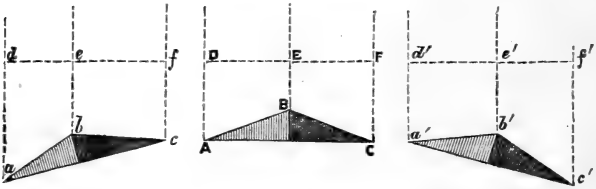
craters, when the central region was at O, we should also be looking at those summits and slopes when the region was at A, B, C, or D,—unless, indeed, the slopes were exceedingly steep.

Of course the two opposite slopes of a ridge, suppose, would be seen in different proportion at A and C, or at D and B, and if they were differently tinted, a different effect would be produced, whether we could see such slopes separately or not.

Thus if such a ridge as A B C (Fig. 2) were looked at directly when at O, we should see the slopes A B, B C, of apparently equal width, as shown by the equality of D E

and E F drawn square across the parallels from A, B, and C, towards the eye; whereas, if the base A C were inclined to the position *a c* or *a′ c′*, we should in one case see *b c* the wider, and in the other see *b′ c′* the wider, as shown by the inequality of the lines *d e*, *e f*, and *d′ e′*, *e′ f′*. Such changes would necessarily produce some effect; and the effect might be deceptive, and indicate change where there had been no

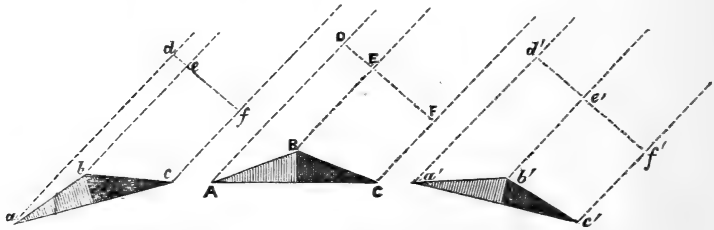
FIG. 2.



change, if a surface were covered with ridges such as A B C, too minute to be individually discernible, and having the side towards C darker or lighter than the side towards A. The same would hold also if the slopes A B and B C were not, as in the imagined case, inclined equally to the base A C.

But it will be manifest that these effects, though they might be appreciable, would be insignificant compared with those which libration might produce, in certain circumstances, on points at a considerable distance from the centre of the disc. Thus, take such a case as is illustrated in Fig. 3, where a ridge, A B C, instead of being looked at squarely, is viewed at an angle of 40°, corresponding to

FIG. 3.



a position removed 50° from O, in Fig. 1. Then lines being drawn from A B C, and, at an angle of 40°, to A C, and parallel lines from the corresponding points of the same ridge tilted on either side of its mean position as before, we see that the inequality between D E and E F is much less than that between *d e* and *e f*, and much greater than that between *d′ e′* and *e′ f′*. The effects of libration may thus be

very considerable indeed on places considerably removed from O (Fig. 1). It is indeed clear that places which, when the moon is in mean libration, are near the lunar limb, but not near enough to be carried actually out of view, must be very importantly affected, since *d e* (Fig. 3) would vanish altogether with a slight reduction of the angle at which the parallels of the figure are inclined to A C.

Remembering that every point of the visible lunar hemisphere undergoes libration, and that in every lunar month there is a complete oscillation of the point O over a certain libration-ellipse, which is continually varying in different months as well in position and size as in the direction in which it is traversed, while the maximum libratory effects (always considerable) are attained at different epochs in different lunations, we see that we have here a very important cause of changes in appearance. It is utterly impossible that any surface like the moon's could, as a whole, undergo such remarkable librations without some noteworthy changes of appearance being produced, even without those changes of illumination which have been referred to above. Further evidence on this point will presently be cited.

Now the cycle of libratory changes for the moon, regarded without reference to her phases, is a long one. It amounts, on the average, to almost exactly six years; and we may say that the same libratory condition is not restored until this period has elapsed. There are momentary coincidences of position, but these positions are arrived at, and passed away from, in different ways, until at the end of the long cycle the same series of changes is re-commenced. But this is not all. We must consider *phase* in this inquiry; indeed, it is the most important consideration of all. Now, the six-yearly period brings back the same libratory condition, but not the same phase when given libration effects are produced. This is manifest if we consider that the node regredes only 19° 21′ per annum, and therefore in six years regredes little more than 116°, having, therefore, a totally different position with respect to ecliptical longitude. In two six-yearly periods it regredes rather more than 233°, or has still a totally different position. In three six-yearly periods it regredes 348° 23′ 24″, or is now 11° 36′ 36″ from its first position. This is the nearest approach. The fourth, fifth, and sixth six-yearly periods bring the node to 23° 13′ 12″ from its first position. We may call this a second eighteen-yearly period. Ten such eighteen-yearly periods bring the node 116° 6′ from its first position, and

one other six-yearly period then brings it into all but perfect coincidence with its first position. But 186 years have then elapsed, and though the conditions are nearly reproduced so far as libration is concerned, the astronomer who made a first series of observations at the beginning of this period is not alive to repeat them under like conditions, even if like conditions existed. Even this, however, is not the case absolutely, since the lunation would be in another part of its progress at any given season of the year, at the end of the long period named.

Of course I would not have it understood that there is not, within much shorter periods, an approach to the restoration of given relations. Two or three times, perhaps, in ten years, any given feature in the moon may be seen under conditions so nearly alike as to produce a great similarity of aspect; and if the weather is favourable on such occasions, a legitimate comparison may be instituted for the purpose of ascertaining if change has taken place. But it may safely be asserted that the opportunities presented during the life of any single astronomer for a trustworthy investigation of any portion of the moon's surface under like conditions are few and far between, and the whole time so employed must be brief even though the astronomer devote many more years than usual to observational research.

I shall now proceed to indicate a remarkable instance of the effects produced by libration on the aspect of a lunar region lying not very far from the limb, in order that the student of the subject may duly recognise the importance of *position* in this matter.

Mr. Webb, in his "Catalogue of Lunar Objects," makes the following remarks respecting a supposed lunar crater:—"On the western edge of the Mare Crisium Schröter delineated a crater, called by him Alhazen, which he employed to measure the existing libration; he saw in it, after a time, unaccountable changes; and now, it is said, it cannot be found. Beer and Mädler confounded it with a crater lying further south; the question, however, which in the interim was debated between Kunowsky and Köhler, is not quite cleared up." In January, 1862, Mr. Birt observed two objects where Schröter had seen a single crater, and addressed a letter on the subject to Mr. Webb, who then gave him the following interesting and instructive history of the region:—"Schröter had watched Alhazen and measured from it for years, and found it too varying in aspect to be accounted for by the varying angle of illumination. At first

it was a depressed circle surrounded by a ring, and distinguished from the neighbouring objects under all angles by its dark grey tint. Subsequently, it often appeared, even in a 27-foot reflector (mirror about 20 inches) under favourable circumstances as a bright longish flat mountain, though *more* frequently in its original grey aspect; occasionally it would be so indistinct, other objects being well defined, that he could not tell what to make of it. On March 1, 1797, Alhazen being very near the limb (only 27·27″ from it), and therefore in a position to be very indistinct, especially as the terminator had advanced to the other side of the *Mare*, he saw it with a 13-feet reflector (mirror about 9 inches and power 180) more distinctly than ever, and in quite a new form, as a real very deep and bright crater, with an irregular ring, scarcely united to the south, and open to the north, with a projection on the east side. Also, there was a small shadow as of a crater never seen before during the innumerable observations of ten years. Schröter thought Alhazen, under this aspect, appeared as deep as Proclus." "Mr. Webb," proceeds Mr. Birt, "enclosed a tracing, with this remark:—'I think you will consider it as affording an interesting comparison with your own observations. He has figured, you will see (as well as described), a little crater where you describe two (?). The circumstance of his ranges uniting so closely to the south may be due to a different libration.' Kunowsky, in the *Astronomische Jahrbuch* for 1825, speaks of Alhazen as lost. In the *Jahrbuch* for 1826, Pastorff, writing January 20, 1823, says his son repeatedly found Alhazen. Pastorff also saw it. In the *Jahrbuch* for 1827, Harding is recorded as having seen Alhazen as Schröter had drawn it. Pastorff saw it in the same year, and in 1829. The difference of aspect, as well as the occasional difficulty of finding this interesting spot, is highly curious. Schröter's earliest delineation gives it, as described, a shallow depression, *entirely* surrounded by a ring. My own observations, which now follow, may perhaps throw some light on these differences and difficulties. Two ranges of mountains, one behind the other, may easily be taken for a crater, and at a certain angle of illumination it may be exceedingly difficult to distinguish the difference. After a while the supposed crater entirely vanishes, libration alters the visual angle, and rotation the illuminating one, the observer being greatly puzzled as to what has become of his well-recognised crater."

Mr. Birt then describes the observations made by himself on January 3 and 4, 1862. On the former day Schröter's

Alhazen at first sight appeared to have somewhat the appearance of a crater, the west edge being high but the east much lower. "Upon attentively considering it," says Mr. Birt, "I have some reason to think it consists of two nearly parallel ranges of mountains, just on the borders of the *Mare*, the eastern range forming a part of the actual border. The shadow of the western range is, under this illumination, terminated at a line west of the eastern range, the western slope of which is glowing in bright sunshine." On January 4, Mr. Birt remarks as follows,—"At times the definition has been very fine, and the real character of Schröter's Alhazen well seen; the southern terminations of the two mountain ranges were seen to be quite separate the one from the other, and the level surface passing between them. It is not surprising that the two combined should have been regarded as a crater, especially if viewed by a low power; for now, haze coming on, it is impossible to distinguish the two from a crater. In the earlier part of the evening, the independence of the two ranges, especially on the south, was very apparent; the *shorter* shadow brought out very distinctly the mountain character, and the recess of the shadow of the eastern range revealed the existence of two (?) small craters lying at the foot of the eastern slope, upon the very border of the Mare Crisium. Beer and Mädler figure two mountain ranges in the locality, but very unlike the mountain ranges described above."

All this is very instructive. It shows what effects the varying visual angle and illumination will produce where most effective, and therefore the effects which they tend to produce under other circumstances.

I pass to the consideration of two lunar regions, to which in recent times attention has been specially directed,—the crater Linné and the Floor of Plato,—proposing to discuss the evidence of change (mechanical change in the one case, and systematic variation recurring each lunar day in the other), with careful reference to all the known circumstances of each case.

On November 17, 1866, Mr. Birt received a letter from Dr. Schmidt, of Athens, to the following effect:—"Depuis quelque tems je trouve qu'un cratère de la lune situé dans le plaine du Mare Serenitatis, n'est plus visible. Cette cratère nommé par Mädler 'Linné' se trouve dans la quatrième section de Lohrmann sous le signe A. Je connais ce cratère depuis 1841, et même en pleine lune il n'était pas difficile de l'apercevoir; en Octobre et Novembre, 1866, à l'époque du maximum de son apparence, c'est à dire un jour avant le

lever du soleil à son horizon, cette profonde cratère, dans le diamètre est 5·6 milles Anglais, était parfaitement disparu ; seulement un lueur, un petite image blanchâtre se présentait au lieu de Linné. Auriez vous bien la bonté de faire quelques observations sur cette localité."

Passing over the earlier observations made in accordance with this suggestion, I quote the statements made by Dr. Huggins, in the "Monthly Notices of the Astronomical Society" for June, 1867. He remarks that "on May 11, 1867, Linné had the appearance of an oval patch on the darker background of the *Mare Serenitatis*. The character of the surface of the white spot may be described as similar in appearance to that of a cloud, for it presented no distinct details, and remained undefined when the small neighbouring craters were seen with great clearness. The absence of any defined points upon which the eye can rest is probably the reason that the boiling motion of our atmosphere is perceived in a much more marked manner over the white spot than on the adjoining sharply-defined parts of the moon's surface. From this cause, Linné appeared on several occasions as a mass of white cloud in motion, at the same time that the craters near it were seen steadily and with distinctness. This cloudy appearance arises probably from a peculiar partly reflective property of the material of which Linné consists. Some other portions of the moon's surface reflect light in an analogous manner. . . . The shallow saucer-like form of Linné was not seen, but I have detected it on other occasions. . . . In the centre nearly of Linné, but rather nearer to the western margin, was seen the small crater. This object was well defined in the telescope. The interior of the small crater was in shadow, with the exception of a small part of it towards the east. The margin of the small crater was much brighter on the western side, and at this part appears to be more elevated above the surface of Linné. Under very oblique illumination, this high western wall appears as a small brilliant eminence, and casts a shadow which is somewhat pointed. . . . I estimated the diameter of the small crater to be rather greater than one-fourth of the diameter of the white spot." . . . . On the evening of July 9, the boundary of Linné was noticed not to end abruptly, but to pass "gradually into the darker surface of the Mare Serenitatis."

Dr. Huggins then proceeds to inquire into some of the historical evidence. "Herr Schmidt," he says, "is of opinion that a great change has recently taken place in the

appearance of Linné, when it is viewed under oblique illumination. This conclusion is based upon a comparison of its present appearance with the descriptions of Lohrmann and Mädler, and with Herr Schmidt's own observations from 1841 to 1843. On this account, it is of importance to notice that the earlier observations by Schröter seem to agree very closely with the appearance which Linné now presents. In Plate IX. of Schröter's "Selenotopographische Fragmente," the place occupied by Linné is marked by a round white spot, and not by the figure of a crater. The spot is distinguished in the plate by the letter *v*. At page 181, Schröter gives the following description of this object:—'Die sechste Bergader Kommt von einer fast dicht an den südlichen Gränzgebirgen befindlichen, verhältlich gezeichneten Einsenkung *u*, streicht nördlich nach *v*, woselbst sie wieder eine ohnegefähr gleich grosse, aber ganz flache, als ein weisses, sehr kleines rundes Flecken erscheinende, etwas ungewisse Einsenkung in sich hat.' . . . The observation was made in 1788, November 5, from 4 hours 30 minutes to 8 hours. The mean time of the observations was 7 days 14 hours after new moon. Schröter employed a power of 161 on his 7 feet reflector. The description of this object as a flat somewhat doubtful crater, which appeared as a round white spot, agrees remarkably with the appearance which Linné now presents under similar conditions of illumination. The absence of any mention by Schröter of the small interior crater cannot be regarded as evidence of much weight that this little crater has been subsequently formed. An object so small might easily have been overlooked by Schröter. However, Lohrmann's description, in 1823,* and that by Mädler, in 1831, do not appear to be in accordance with Schröter's observations, or with the present aspect of the object. My observations were made with a refractor of 8 inches aperture, and with various powers from 200 diameter to 800 diameters."

The next communication from which I shall quote is contained in a paper in the *Student*, August, 1867, and is by Mr. Birt:—

"The question of change on the moon's surface, supposed to have been manifested in the case of the crater Linné, with which our readers are acquainted, remains *undecided*. Respighi, on the Continent, as well as several eminent astronomers in our own country, having come to the conclusion

* "A is the second crater upon this plain,—has a diameter which exceeds somewhat one mile, is very deep, and can be seen under every illumination."—*Topographie der Mondoberflache*, p. 92 and plate, section IV.

that no change whatever has taken place in the condition of Linné, and that if any appearances have been presented indicating change, such appearances are to be explained either by defective observations, by unfavourable conditions of our own atmosphere, by variations in the angles under which we see lunar objects, or by different incidences of the solar light falling upon them.

"The results that have as yet been arrived at, and which are supported both by English and Continental observations, are as follows:—

"First. The existence of a *shallow* crater, usually presenting the appearance of a whitish cloud, which, by the way, is of variable size; the crater itself has been very rarely seen. Respighi saw it on the 10th of May, 1867, during a perfectly tranquil state of the air. Knott caught a sight of the ring on January 12th, 1867, and, on the same evening, in moments of quiet air and good definition, Buckingham noticed the shallow depression. Webb saw the ring on April 11th, 1867.

"Second. In this shallow crater or depression, a little west of the centre, a small crater with a well-marked interior shadow has been seen more or less distinctly, both in England and on the Continent, since November, 1866; in some cases a perfect crater, in others portions only have been detected. The evidence tending to establish the existence of this small crater is certainly beyond dispute.

"Third. Herr Schmidt, of Athens, carefully observed Linné from October 16th, 1866, and during November, 1866, without having detected either the large *shallow* crater or the small one within it. The rim of the small crater appears to have first arrested his attention on December 13th, 1866, as a delicate white hill; Buckingham seems to have first seen the shadow as a black spot on the following evening, December 14th.

"From the data given, a table (see next page) has been constructed.

"Respighi would seem to have measured the shallow crater instead of the small one."

Wollff makes the following remarks in the *Comptes Rendus* for June 17, 1867:—

"Since the 10th May I have noticed that the crater Linné continues to exist, but with a much smaller diameter than that of the crater indicated in the maps of Lohrmann or Beer and Mädler. In the centre of the white spot a circular black hole may be seen, bordered on the west by a portion of ground which seems prominent above the

ESTIMATIONS AND MEASURES OF THE EXTENT OF LINNÉ.

Authority.	Epoch.	Eng. feet.	Seconds.	Objects.	Remarks.
Schmidt . .	—	36,449	5·17	Crater.	
B. and M. .	1831	33,482	4·83	Crater.	
Schmidt . .	1866, Oct. 18	48,688	6·90	Whitish cloud.	
Birt . . .	1866, Dec. 15	81,920	11·61	Whitish cloud.	Measured.
„ . . .	1866, „ 18	49,886	7·07	Whitish cloud.	Measured.
„ . . .	1866, „ 19	51,650	7·32	Whitish cloud.	Measured.
„ . . .	1866, „ 21	47,627	6·75	Whitish cloud.	Measured.
Schmidt . .	1866, „ 27	12,790	1·81	Whitish cloud.	
Birt . . .	1867, Jan. 14	56,100	7·95	Whitish cloud.	Measured.
Buckingham	1867, Mar. 14	42,336	6·00	Whitish cloud.	Measured.
Wolf . . .	1867, June 12	31,752	4·50	Whitish cloud.	
Birt . . .	1867, July 8	37,623	5·33	Whitish cloud.	Measured.
„ . . .	1867, „ 9	49,420	7·00	Whitish cloud.	Measured.
„ . . .	1867, „ 10	37,845	5·36	Whitish cloud.	Measured.
Schmidt . .	1866, Dec. 13	1,918·4	0·27	Delicate hill.	
„ . . .	1866, „ 26	1,695	0·24	Fine black point.	
„ . . .	1867, Jan. 25	1,279	0·18	Fine black point.	
„ . . .	1867, „ 25	1,918·4	0·27	Fine white peak.	
Secchi . .	1867, Feb. 11	2,352	0·33	Small crater.	
Respighi .	1867, Apr., May	28,224	4·00	Small crater.	
Wolf . . .	1867, June 12	7056	1·00	Small crater.	

remainder of the spot. This slight extra elevation has already been described by Schmidt. Atmospheric circumstances did not allow me to obtain an irreproachable image of the moon before the 10th June. On that day, at 8 o'clock, Linné had already been in full light nearly 48 hours, and the central hole could be seen with perfect sharpness. It is a deep crater—deeper than most of the little craters surrounding it, if one may judge from the comparative intensity of the shadows; but its diameter is not equal to that of craters A and B of Beer and Mädler. The white spot which spreads radiatingly (*s'étend en rayonnant*) round it, had, on the 12th June, a diameter of 4·5″, that of Bessel being 7·7″; the crater itself subtending a little less than one second. The perfect purity of the atmosphere, and the optical power of the telescope which I employed, allowed a number of small craters to be seen very distinctly round Linné, or rather a number of small round holes without elevated margins, and which are not shown in Beer and Mädler's map. Six of these little craters form a very remarkable double range to the north and north-east of Linné. They are smaller than the craters in a line situate to the north-west of Linné, and noticed by Schmidt. I employed magnifications of 235, 380, and 620 times.

"The brightness of Linné has not changed since Beer and Mädler's observations, for it is always equal to that of the white spot situated near Littrow, on the western margin of the Sea of Serenity, to which B. and M. assigned the luminosity 6.

"If, then, we compare the actual appearance of Linné with the *text* of Lohrmann and his successors, it is possible, *à la rigueur*, to believe that it has undergone a certain change. Linné has always been a deep crater, with elevated margins; its lustre has not changed—its total diameter has remained about the same. A comparison of *maps*, on the contrary, indicates a real alteration, for these figure a large crater occupying all the space now filled by the white spot. Schmidt thinks that we cannot refuse to attribute great weight to the identity of the indications of these two maps. The authors of the second, having the first at their disposal, it is probable that if they had not found the great crater drawn by Lohrmann, they would have noticed so extraordinary a fact. It is not, however, without interest, to compare their indications with that of earlier maps. The picture drawn and presented by Lahire, which is in the library of St. Geneviève, represents Bessel, Sulpicius Gallus, and other little craters, equal to Linné in the map of Mädler; but he does not indicate Linné. He has only many white spots in this part of the sea. Cassini's map appears merely a copy of Lahire with less detail. According to Schmidt's note, Schröter seems not to have seen Linné, at least not as one of the principal craters in the Sea of Serenity, although he noticed others that were smaller.

"If we consult the photographs of the moon, we see, in the large copy of Warren De la Rue (1858), Bessel and Sulpicius Gallus exhibiting an indication of an interior shadow, while Linné figures as a white spot. The same is seen, though clearer, in the enlarged copy of the magnificent photograph obtained by Mr. Rutherford on the 4th March, 1865.

"The disappearance of the great crater of Linné, then, dates as far back as 1858, if not as far back as Lahire. Apart from the indications supplied by the maps of Lohrmann and Beer and Mädler, to which we may oppose the counter indications of Lahire and Schröter, we only possess a single positive document testifying that Linné has undergone any change, and that is the affirmation of Schmidt that his crater and drawings of 1841 represent the object differently to what is now seen."

In April, 1869, Mr. Browning described his observations of Linné, and gave pictures of the shallow crater and the small crater within. These pictures are interesting, as showing the changes which the same object may present, as seen by the same observer, within a very short space of

time. The following description is from Mr. Browning's Note-Book. I invite special attention to the times of observation:—

"September 8, 1869. 11.30 a.m.—Saw Linné very distinctly as a crater, elongated north and south. The north part of the crater the narrowest. The evening terminator crossing Bessel. Eye-piece used, positive achromatic, power 208.

"2.15 a.m.—The wall of the crater appears nearly perpendicular on the west.

"3 a.m.—The small crater is seen to be in the centre of a shallow cup, about four or five times its own diameter. Eye-piece same kind, power 306.

"4 a.m.—A white nebulous line, resembling steam, appears to start from the mouth of the crater, and is continued in the form of a scythe, with the blade directed towards the west. At 4.15 this line was much fainter, and at 4.50 it was no longer visible. As this appearance only presented itself for about ten minutes, I have little doubt that it was an optical illusion, caused by a small cloud in our atmosphere, moisture in front of the pupil of the eye, or some cause quite independent of the object." (I venture to doubt whether any such cause could have produced an illusion of the sort lasting for so many minutes). "During the time the appearance lasted," proceeds Mr. Browning, "I changed the eye-piece, and rotated the drawer-tube which carried the eye-pieces. As neither of these proceedings affected the appearance, it could not have been a 'ghost,' caused by reflection from the surface of one of the lenses in the eye-piece."

Mr. Browning, after considering the evidence afforded by his own observations, considered in connection with those made by others, arrives at the conclusion that "there is scarcely any ground for supposing that any change has occurred in this small but celebrated crater. Should this eventually prove to be the case," he adds, "the time that has been given to this matter has not on that account been lost. Attentive examination of this minute object has made us acquainted with peculiarities of the reflecting power of portions of the moon's surface which may ultimately lead to some more exact knowledge of the character of the surface of our most interesting because most perplexing satellite."

These remarks appear to me to contain the gist of the whole matter. We see that Linné has a surface so constituted that as the sun is rising there, and so pouring his rays

very obliquely, there is a continual change of aspect precisely resembling that which can be recognised when certain kinds of rock surfaces, and especially crystalline formations, are viewed under oblique illumination. We know that in such cases the tints vary not only absolutely, but relatively, insomuch that a part which is darker than another with one oblique illumination will be lighter under another and but slightly different oblique illumination.

It appears to me that no other explanation can reasonably be suggested; because, in point of fact, we have to choose between the theory that there has been a definite change of surface in this part of the moon, or that the change is only apparent. Now if there has been a definite change at any time, fresh changes must have restored, either from time to time or definitely, the former condition of the surface. But this seems extremely unlikely; while such a change as Sir John Herschel considered to afford the best explanation of Schmidt's observations may be regarded as one which no subsequent process could so modify as to restore, or nearly restore, the original appearance of the region. For, says Herschel, "the most plausible conjecture as to the cause of" the disappearance noted by Schmidt, "seems to be the filling up of the crater from beneath by an effusion of viscous lava, which, overflowing the rim on all sides, may have so flowed down the outer slope as to efface its ruggedness, and convert it into a gradual declivity casting no true shadow." Such a change would doubtless account well for the observed appearances; but it leaves the restoration of the crater afterwards unexplained.

It remains to be noticed that recently the crater has been observed again somewhat attentively by Dr. Erck and Mr. Burton. Mr. Birt thus speaks of their observations of Linné:—"During the present year this celebrated object has appeared more of a crater form than for some years previously, at least since 1866. . . . On June 4, 1873, Dr. Erck measured the largest diameter of the elliptical spot, and found the mean of several measures to be equal to 4 seconds, or 28,226 English feet, which but little exceeds half the length of the largest diameter as measured by Dr. Huggins. Mr. Burton gives a drawing to scale, which differs very materially from Dr. Huggins's, in the "Monthly Notices." In Dr. Huggins's drawing, the small crater is situated in the *western* part of the white spot, its exact position being indicated by the following numbers: Length, 35; small crater, 5, or one-seventh of the length of the white spot; west rim of small crater distant from west edge

of white spot, 10; east edge of small crater distant from east edge of white spot, 20. In Dr. Erck's (and Burton's) drawing, the small crater is situated on the *eastern* part of the white spot; its position, measured on the same scale as Dr. Huggins's drawing, is shown as follows: Length, 15; small crater, 5, or one-third of the length of the white spot; west rim of small crater distant from west edge of white spot, 8; east edge of small crater distant from east edge of white spot, 2. Upon reducing the numbers for each drawing to the same scale, we have, in 1867, the eastern distance double the western, and in 1873, the same distance is only one-third that of the western. Equal weights being accorded to the drawings (and we know that, in 1867, the then president of the R.A.S., Professor Pritchard,* laid great stress on the drawings of Dr. Huggins), it is clear that Linné has undergone a change in the interval between 1866 and the present time, and this circumstance of itself is enough to induce renewed energy in as earnest an attack upon Linné as took place in 1866 and 1867, especially as there is now great probability of settling the disputed question of change."

It needs, however, only a comparison between the three drawings taken by Mr. Browning on September 8, 1869, to see that within the space of three hours Linné changed from the aspect presented to Mr. Burton in 1873 to an aspect more nearly resembling that presented to Dr. Huggins in 1867.

I pass now to the Floor of Plato.

It was in November, 1861, that Mr. Birt communicated his first series of observations of Plato to the Royal Astronomical Society. On the same occasion he described an instrument for comparing colours, which he proposed to call the *homochromascope*. In describing this instrument, he remarked that he had "found it necessary to devise some means for comparing with various standards of colour the tints of various portions, especially the dark-floored craters, the extensive grey plains, and the more luminous districts in immediate proximity with the raised craters." I do not quote the description of the instrument, because I am only concerned with its purpose; and, in point of fact, the instrument has not been completed, nor any substitute

* We have evidence here of the mischievous results which must follow from the election to high office in a learned society of a fellow thereof who, whatever his qualifications may possibly be in other departments of knowledge, has no special knowledge of the subject to which such society is devoted. The supposition that any astronomical work by Dr. Huggins could gain weight from comments by Professor Pritchard is amazingly absurd.

used in the observations of Plato, so far as I know. What I wish to indicate is that, at the beginning of this inquiry, Mr. Birt recognised the necessity of some instrumental contrivance for determining colour tints, as distinguished from mere processes of eye-estimation.

The observations of the Floor of Plato divide themselves into two classes. First, there is the observation of the floor itself, regarded as a whole, and compared with neighbouring regions, and especially with the neighbouring parts of the *Mare Imbrium;* then, secondly, there is the study of the spots, thirty-seven in number, which have been detected in the floor, and which vary in visibility, not only absolutely but relatively.

The main result which Mr. Birt deduces from the general study of the Floor of Plato is that it grows darker as the lunar day advances, being darker near noon at Plato. I do not know that any good would be gained by entering into minute details, which would occupy much space, and would not elucidate the subject, since those who believe that a real change takes place attach no importance to these minutiæ, but ask for acceptance only of the general fact that as the sun rises higher above the level of Plato the floor of the crater grows darker as compared with the neighbouring region, and that the morning tints are resumed as the sun gradually passes from its culmination descendingly towards the western horizon of Plato.

I have before me as I write a series of observations made by Mr. Neison, F.R.A.S., which would occupy, were I to quote them in full, far more space than remains available to me. They indicate, as satisfactorily as need be, the process of darkening to which I have just referred.

Nothing could seem more clearly demonstrated than the fact that Plato darkens towards the noon-tide hours of that lunar region. We might enter on the inviting speculations suggested by such a state of things, inquiring whether that darkening was due to some process of vegetation, or to chemical changes in the surface of the floor. And a variety of speculations more or less ingenious might suggest themselves as to the general condition of the regions within the circular crater-walls.

But we must not overlook the possibility that the darkening of Plato may be apparent only, not real. It is most important that before we begin to reason on processes of change we should assure ourselves that change really does take place. There is another reason for caution in the circumstance that, so far as can be seen, any explanation

of the darkening of Plato which refers the phenomenon to real processes of change, must be based on the conception of physical processes unlike any with which we are familiar; and it is a recognised rule in science that such conceptions should be avoided. It is true that in regions to which we are unable to extend experimental research such processes may take place; nay, we may say that certainly there occur in nature many kinds of action which are unlike any we are familiar with. But it is one thing to recognise such things as possible or probable, another to accept them as legitimate explanations of observed facts. Now, processes resembling vegetation, recurring within a period of a few days, taking place in an atmosphere more tenuous than the vacuum of a receiver, repeated after a fortnight of intense cold, and brought about by as remarkable an intensity of heat, must be regarded as quite beyond our experience, and therefore affording a very unsafe basis for reasoning, to say the least. Nor is it a fact unworthy of being noticed that the apparent maximum effect on the Floor of Plato occurs at the noon hour of the place instead of at the hour corresponding to two o'clock in the afternoon, when, according to our experience, the accumulated effect of solar action is greatest. Then, again, if we compare the darkening of Plato to some of those chemical processes with which we are familiar as effects of solar action, we find the change after lunar noon at Plato unintelligible, since assuredly any chemical process progressing as day advanced should not come to an end at noon and then be replaced by the reverse process, but would continue until the evening hours, and according to our ordinary terrestrial experience would leave a permanent effect.

Setting aside other possible explanations—for it is a mistake to suppose, as Mr. Birt appears to do, that surfaces of different tints must maintain the same relative tints under varying illumination—there is the effect of contrast to be considered. This I believe, from my own observations, to afford the true explanation of the observed phenomena. Plato lies on lunar highlands, which shine very brilliantly under high solar illumination. Towards the Mare Imbrium a comparatively narrow ridge separates the floor from that region. Now when the terminator has just passed beyond Plato, the surrounding wall is not nearly so bright as at the time of full moon; the black shadow of its western ridge occupies the western side of the floor; and the eye, in estimating the tint of Plato, is neither oppressed with the glare of general light on the one hand, nor forced to

compare the tint of the floor directly and solely with a much brighter surface; if the comparison is made on the east, with an illuminated wall, it is made on the west with a perfectly black shadow-streak. Similar remarks apply to the time when the terminator is about to pass away from Plato. But at the time of full moon, the highlands around Plato are very brightly illuminated. The glare necessarily makes Plato itself look relatively dark, notwithstanding the fact that the floor is also much more brilliantly illuminated; for it is a recognised fact, that surfaces of unequal light-reflecting capacity appear to differ more in brightness under a high illumination than when they are only faintly illuminated. We know, in fact, that a surface which is only dark looks almost or perfectly black when itself and a bright background are under strong illumination.

We have recently had some remarkable illustrations of the deceptive effects of contrast in the aspect of Jupiter's fourth satellite as it has crossed the face of the planet. This satellite is somewhat inferior in light-reflecting capacity to the other three satellites, and if it were not for the physiological law into which I am now enquiring, we should expect this satellite to look somewhat darker than the others when transiting the disc of Jupiter. But as a matter of fact, instead of looking merely dark, this satellite looks nearly black when on Jupiter's face, insomuch that it has been often mistaken for a shadow of a satellite. Mr. Roberts, when observing one of those dark transits of the fourth satellite, could scarcely believe that the satellite would be visible at all when outside the disc; and yet, on every such occasion, as soon as the transit was over, the satellite was seen as usual, though, when close to the planet, looking rather faint by contrast. Mr. N. E. Green, a very excellent observer, and to whom we owe some admirable pictures of Mars and Jupiter, made the following even more pertinent observation on the 26th of March last:—"The transit of the fourth satellite," he says "was closely observed; it certainly appeared as dark as any of the shadows, sometimes even sooty in its blackness, and on leaving the disk seemed unusually faint. But here a remarkable fact, in connection with the law of contrasts, was observed. No sooner had it passed away into the clear sky than it seemed to be brighter than the dark belt against which it had so recently appeared as a decided dark." Such an observation as this appears to me to be decisive against mere eye-estimations, showing that absolutely no reliance can be placed on them unless some

contrivance is employed to destroy the effect of contrast. As it is conceded that none of the observed darkenings of the Floor of Plato have been other than eye-estimates, no further discussion seems needed, or can in my opinion be legitimately given to the subject. I may mention, however, that having, though as yet in an imperfect manner, studied Plato with a much reduced field, so that I could eliminate to some degree the effects of contrast,* I have not found that the floor grows relatively darker towards the time of full moon.

The other circumstance which has been referred to change, the variation of the visibility of the spots on the Floor of Plato, cannot be explained as due to contrast, since, in point of fact, the study of different spots introduces a correction for any effects of contrast. Contrast might make any given spot more conspicuous at one time than at another; but it could not cause one spot to be visible at one time when another was invisible, and then after a time the latter to be visible when the former was unseen. As this is what has been detected in the spots on the Floor of Plato, we must either believe that a real change takes place in these spots, or else we must adopt some optical explanation other than that depending on the laws of contrast.

There is a difficulty in the assumption of real change in these spots which does not present itself when we are considering the change of a single crater, or of the floor regarded as a whole. It is not easy to imagine any processes by which different spots in the same limited region of the moon, and under like circumstances, should be very differently affected. If these spots were vegetation-covered, we should expect them to show similar variations; or if we admitted the possibility of different effects, resulting from the same general process, we should still require evidence showing that the changes of visibility had as a period either the lunar day or the lunar year, neither of which relations has in any single instance appeared; and similar results follow whatever real changes we imagine to have affected these regions.

But if we enquire into apparent changes taking place, we find a case precisely corresponding to that of Linné. In Linné the white spot assumes varying proportions and dimensions with varying illumination, and doubtless also

* In point of fact, the field was larger than Plato; but I was able to get the boundary of the field across the floor, and thus to produce effects corresponding to those at sunrise and sunset, when there is shadow on the western or eastern side of the crater.

with varying position as libration operates to shift the region. Now the aspect of the Floor of Plato, as seen with a powerful telescope, suggests precisely that condition of the surface which would render its appearance most likely to be affected by such changes. The floor is in a general sense level, though it does not suggest the idea of smoothness. It looks as if it were granulated, and the streaks and spots present the appearance of having a surface not differing in tint alone, but in texture; or rather (such at least as been my own opinion when I have studied the Floor of Plato with Lord Lindsay's $12\frac{1}{4}$-inch telescope) they suggest the impression that their difference of tone is due much more to difference of surface-texture than to difference of tint. Now we view the Floor of Plato very obliquely, the line of sight having a mean inclination of only 40° to the surface, and libration therefore affects the direction of vision very importantly, according to the principle indicated above. The range, in fact, roughly is, from an angle of 33° to one of 47°, or the greatest and least angles are nearly as 3 to 2. It would not be at all wonderful, therefore, if a surface such as the Floor of Plato varied greatly in appearance—now one, now another part being the darker.

The following experiment should be tried by those who imagine that the unequal affections of such spots as are on the Floor of Plato manifestly indicate real change. Let a flat circular tin be filled with sandy earth, and over parts of this earth let drops of different liquids be let fall, some colourless, some slightly tinted, some drying with a glazed surface, and so on. Let also certain spots be formed on the surface, by removing portions of the sandy earth and substituting finely crushed glass of various neutral tints. Now let the general surface be viewed at an angle of about 40° (that is, the line of sight inclined 50° to the normal to the surface); then (1) let the direction of illumination be varied, while the direction of sight remains unchanged, and (2) let the direction of the line of sight be changed through 6° or 7° on either side of the original angle of 40°. I venture to affirm very confidently that the behaviour of some of the spots will satisfactorily prove that apparent changes of relative brightness are no sufficient evidence of real changes in the nature or condition of a surface.

The mistake seems to me also to have been made of supposing that, because a lunar surface looks smooth, or because the terminator when crossing the surface shows no indentations, therefore the conditions of illumination

may be discussed as though the surface really were optically smooth. In point of fact, if a surface were covered over with minute cones a quarter of an inch in height, it would present the same peculiarities of general illumination as though it were covered with conical hills several hundred feet in height. Now it may be said that as no such irregularities, whether large or small, can be detected in the Floor of Plato, it is not admissible to make the possible existence of such peculiarities a basis of reasoning. That is perfectly just; but it is equally inadmissible to make the possible smoothness of the Floor of Plato a basis of reasoning. We have no direct evidence either way. As to probabilities, it seems at least as likely that the floor is covered with minute elevations as that it is smooth; *nay*, if we remember that the floors of the lunar craters present all the appearance of having once been fluid with intensity of heat, it seems more reasonable to infer that their surfaces now have a crystalline structure than that they are smooth.

We may, indeed, sum up the evidence obtained by Mr. Birt in this way:—It implies either real changes, or surface peculiarities, probably of the nature of minute irregularities, such as result from crystallisation. If real changes be regarded as very unlikely, we have strong probable evidence in favour of surface peculiarities, a result of considerable interest. If surface irregularities be thought very improbable, then we have strong evidence in favour of real changes, a result also of considerable interest. Whether it is more unlikely that real changes so affect these spots as to make their whole surface change in brightness (though no large surfaces ever show such changes), or that the once fluid surfaces within the craters should have a granulated surface different in different parts, is a question which will be answered according to the general views of the lunar student. I have no hesitation in adopting the second view as far the more probable.

I would not, however, have it understood by any means that I think it unlikely that change is taking place on the moon's surface. On the contrary, when one considers the wide variations of temperature to which the surface of our satellite is exposed, it seems altogether probable that (1) a process of disintegration must be in progress, which should at times, one could suppose, produce even remarkable catastrophes on the moon's surface; and (2) that the lunar atmosphere, tenuous though it undoubtedly is, may be affected by changes of condition detectible under certain conditions from our distant standpoint.

Still less would I have it thought that such researches as those which Mr. Birt has advocated, and to some degree prosecuted, should be discontinued as useless. On the contrary, I consider in the first place that they have already led to results which, however interpreted, are of great interest and importance, and that in the second they are the only possible means for arriving at a solution of the problems suggested by the general aspect of our satellite.

What I wish, however, to urge earnestly on students of the moon is the necessity of perfect independence of preconceived opinions as they proceed in their enquiries. The results they obtain should not be held to owe their importance to the evidence they seem to give in favour of this, that, or the other theory, but should be discussed altogether without bias for one view or another. If in this essay I have given prominence to the objections which may be urged against the evidence thus far obtained, so far at least as it has been regarded as evidence of real change, it is solely because, in my judgment, this is necessary to correct a contrary tendency on the part of those who consider that real changes have been demonstrated. I shall venture to quote, in conclusion, the words of a well-known student of the moon, who, if he inclines somewhat (as his words will show) to the theory that change has taken place, nevertheless presents very fairly the doubts which really surround the whole subject. Thus, then, speaks the Rev. Mr. Webb, after describing some of the principal causes of deception by which changes of illumination, or of direction of vision may affect the aspect of lunar regions :—

" It would be easy to extend this list of causes of deception; but those here given may suffice as indications of the caution with which it is necessary to approach the much disputed question of still existing physical change. In the answer to that question—the affirmative answer—undoubtedly lies a great part of the charm of selenography. Whatever may be the magnificence of the abrupter features of the lunar scenery, or the smooth and tranquil aspect of its gentler valleys and wide-extended plains, we shall contemplate them with a different amount of interest accordingly as we are obliged to consider them an inanimate and silent record of the worn-out and spent convulsions of bygone ages, and forces wholly extinct in selenological death ; or whether we may detect if it be but the last feeble efforts of that marvellous working which once threw open such amazing gulfs, and piled up such terraces and towers

and pyramids, and overspread such wide-extended areas of the globe with confusion and ruin. Some observers may have perhaps been precipitate in assuming the utter and final collapse of all those ancient and evidently long-enduring energies. It were safer to wait and see whether all is indeed so dead and cold. And again, we must not assume, we have to prove—if it can be proved—the absence of atmospheric phenomena. This is not one of those cases where an undemonstrated negative may suffice. The burden of proof—or rather disproof—here naturally rests upon the opponent, when all analogy is in favour of some kind of gaseous envelope: and whatever may, or rather must, be its tenuity as compared with our own, its total absence would be contrary to all chemical and mechanical probability. Nor is it a mere theoretical question: indications are not wanting that the inferences of Schröter and Gruithuisen, to whatever exception they may be liable in their full extent, are at any rate deserving of some consideration. We may be called upon to make abundant deductions on the score of precipitancy and prepossession, and yet a residuum may be found to exist, small in amount, but refractory in character, which cannot be disposed of by any summary mode of treatment. Simple negation will not suffice, much less contemptuous neglect of the labours of those who have preceded us. The first general aspect of that great world lying in its confusion and desolation may indeed be, to some eyes, that of absolute quiescence and arid sterility; a wilderness of rock and sand, lifeless and even soundless, in its unclothed contact with the emptiness of boundless space. But the student, in proportion to his earnestness and perseverance, may see cause to be distrustful of first impressions; he will rather be looking out carefully for those minute indications—and experience has proved that only minute ones can be expected—which may yet show to a well-trained eye and cautious judgment that such a conclusion would be too precipitate. At any rate the question is not yet set at rest; and it can only be finally decided by the faithful carrying out, in very circumstantial detail and with scrupulous accuracy, of the graphical representation of the moon's surface."

NOTICES OF BOOKS.

THE PYRAMIDS OF EGYPT.*

A NEW work on the Pyramids of Egypt has been produced in the present year in France, by M. Dufeu, Member of the Egyptian Institute, and of the Society of Historical Studies of Paris.

It is a goodly-sized octavo, of 322 pages, and claims to have discovered the true object, end, and aim of the "Four Pyramids of Gizeh;" by means of methods, too, and strange discoveries, which are entirely new to the world: while the chief results thus attained to, are that—

(1). Menes ascended the throne of Egypt at the date of 5641 B.C.
(2). The Great Pyramid was founded in 4862 B.C.
(3). All the Pyramids are scientific monuments, and scientific only; and—
(4). The authors of the designs of all of them were the priests of the profane idolatrous Egyptian religion.

The names of Champollion, De Rougé, Mariette, Lepsius, and other well known scholars and *literati* occur so frequently through the pages of this dazzling book, that one might at first expect the author to be of the school of the modern Egyptologists; but that is far from being the case, for there is no deciphering of any piece of hieroglyphics all through the work; and its chief method of proceeding is the wonderful assertion that the list of Manetho, so long held to be a chronological and historical account of the kings of Egypt, is in reality nothing but a series of readings (in an arbitrary unit) on the scale of the Nilometer near Cairo, of the successive annual inundations of the Nile, combined with occasional geological variations of the level of the land through which it flows: a wild theory which no Egyptologist has yet been found venturesome enough to take up with.

Again, there are so many quotations of figures representing either cubits, metres, feet, or inches, and so much assertion of science existing in the Pyramids,—that some persons might imagine that the author is a partaker in that particular "scientific theory," which was commenced by the late John Taylor, in London, and has been carried on since his time to farther developments by many other workers, including myself. But that is also far from being the case: for M. Dufeu's results in chronology, science, and religion are perfectly different from

* Découverte de l'Age et de la veritable destination des quatre Pyramides de Gizeh, Principalement de la Grande Pyramide. Par A. DUFEU. Paris: Ve A. Morel et Cie., 1873.

those of the English scientific and *sacred* theory; and his methods of proceeding are distinguished far more by ignorance than knowledge, of the absolute local and monumental facts.

The very title of M. Dufeu's book is a senseless antagonism to Pyramidal measured and known data, when he therein speaks of "the *four* Pyramids of Gizeh;" for who, after once seeing the Pyramids of Gizeh (Jeezeh), would ever think of speaking of them as *four*, when there are three large ones and two sets of three small ones each, or nine altogether? *One*, he might speak of, because one of them is larger and better than all the rest; or *two*, he might name, because the second one is so nearly as high as the first that many old Arab authors alluded to them as "the pair;" and *three* he might talk of because the third, though by no means so large as either the first or the second, is yet far larger than all the others, stands in a line with the first and second, and is thought by some authors to have been even more expensive than them in construction, on account of the large quantity of granite employed in its casing, making it through all history "the coloured Pyramid." But the moment any one passes beyond this third one, he must, to be truthful, either mention the six small ones, little mites of things though they be, or none at all.

And then behold the "science" of the book! M. Dufeu asserts that the Great Pyramid commemorates its own longitude! But how, and from what, and why?

By taking the profane cubit of Egypt, dividing it into 360 little parts, and representing in those previously unheard of and unused terms, an unimportant feature of the unfinished, subterranean chamber of the Great Pyramid, adding, multiplying, and dividing by other arbitrary numbers representing the rise of some Nile inundations at some time or other, the author at last gets a number which, he says, enables him to state that the longitude of the Great Pyramid was 152° 38′ 20″ from a certain point in the 44th degree of latitude; but whether that latitude was in the Northern or Southern Hemisphere, and whether the meridian was east or west of the Great Pyramid, he finds nothing to define. So there are four points to choose amongst; and as three of them fall in deep ocean water, but the fourth happily alights on land in the American continent, in the Oregon district—the author adopts that point, and exclaims "*therefore* America had been discovered and known by them (the Pyramid builders) before the foundation of the Great Pyramid of Gizeh, 6735 years ago" (see page 210). Yet the excellent M. Dufeu never seems to have considered that if the longitude of the Great Pyramid was measured from a point in America and not that point in America from the Great Pyramid, if there was any longitude measuring at all—America should have been a more civilised, advanced, and developed country than Egypt, in that very early day, and have left behind it some marvellous

trophies of science and art, philosophy, history, and literature, both sacred and profane, dwarfing utterly in their age all the remains that have come down to us from all the "five great empires of the East."

Next, let us take an example of ignorance of Pyramid surface facts of a very important class. In his chapter 14, the author arrives at the conclusion, that "the Great Pyramid was never cased," or coated with the much talked of sheet of smooth, bevilled casing stones; and because, he says, there is not only no such casing now to be seen, but no fragments even of it; none of the prismatic edges or corners of the stones, which would have been knocked off at the place, if the casing stones had been pulled down by the early Caliphs, and carried away to build Cairo, as Arab tradition reports was done. Of such fragments M. Dufeu has the hardihood to declare—

> *On n'apercoit pas la moindre indication, la moindre trace de débris de nature a révéler un pareil travail;—*

and again—

> *rien de cela ne s'apercoit.*

Yet I, having carefully examined in 1865 the four enormous heaps of rubbish on the four sides of the Great Pyramid, have found them almost entirely made up of fragments of the peculiarly white Mokattam limestone employed for the casing stones; and have further been rewarded by finding many of the "prismatic" corners and edges of casing stones, and nothing but casing stones, giving under measurement the very characteristic and crucial angle of the slope of the sides of the Great Pyramid. A collection of these angular fragments I had the honour of presenting, in 1867, to the Royal Society of Edinburgh, where they may be seen in a special case in their Museum: while only last year the interesting present was made to me, by my friend Mr. Waynman Dixon, C.E., who was then at the Great Pyramid, of an almost complete casing stone, which he had found amongst the rubbish on the north side of the Great Pyramid's base, together with large fragments of several others. Mr. W. Dixon's completer specimen measures 25 inches long, 21 high, 30 thick, has the outer bevilled slope of 51°, 51′±, and is now in the official residence of the Astronomer Royal for Scotland: a solid witness to the absolute folly of M. Dufeu's assertion, and a proof of his utter ignorance of the most essential facts connected with the exterior of the Great Pyramid.

But now for more curious things touching the interior.

In chapter 17, the author discusses the coffer or sarcophagus, in the king's chamber inside the Great Pyramid. In so doing, he speaks of it as "a box;" considers it to be a representation, by means of outside and inside measures, arbitrarily multiplied and divided by him, merely of the length of the profane

Egyptian cubit; in illustration whereof he gives, in his table 5, five different sets of measures of the said monolithic stone box, coffer, or sarcophagus.

Of these sets of measures, the first one is intituled as being "by the computation of Piazzi Smith and Joseph Jopling."

Now, if the first name is intended for mine, I have to say that Joseph Jopling is now dead, and I never saw him during life, and never worked in concert with him in anything; and though I have read some of his writings, I never approved of his hypothetical ideas about the size of the coffer, but, on the contrary, maintained that they were very erroneous. What I have, at any time, published for the size of the coffer as my own results were the means of very numerous measures taken by myself at the place—more numerous, indeed, than all the published measures yet taken by anyone and everyone in modern times (see my "Life and Work at the Great Pyramid," vol. ii.) and very different in result or amount from the numbers attributed to me by M. Dufeu, with what object I do not pretend to know.

The fourth column of measures set forth by M. Dufeu is labelled as being by Professor Greaves, the Oxford Astronomer, whom the author seems to consider a very late authority on the Pyramid, and a great deal more trustworthy than me; yet he died two hundred and thirty years ago, and all his measures were taken merely at a single visit on one particular day to the Pyramid, with a janissary guarding the entrance all the time.

The fifth set of measures M. Dufeu entitles "notre mesure;" and yet, such title notwithstanding, I am compelled to dispute that the numbers which he gives could really have been measured upon the coffer of the king's chamber in the Great Pyramid, by any "savant" whatever.

My special reasons for thus declining to accept the statement of a Member of the Egyptian Institute, are, that if the gentleman himself, or any friend of his, had really measured each of the six elements of the coffer, as he has recorded them, to 0·001 of an inch, he could not have failed to have discovered that one side at one part of its height was longer than the other by a whole inch; that three of the sides are curved and not flat; and that there is a *quasi* sarcophagus ledge of large size cut out of the substance of the top of all the four sides, raising very serious questions as to how the measures are to be taken; yet not one word on any of these most noteworthy features is there throughout the whole book.

In conclusion, though the author is on every few pages declaring that his theory is entirely new, and that he is therefore a hero to publish it, I am sorry to say that it is not more new than it is true; for the greater part of it was invented, written, and printed for private circulation in 1863, by Hekekyan Bey, an old Armenian officer now resident in Cairo; was discussed

with him there by me in 1864, and noticed as untenable in my "Life and Work" book published in 1867.

M. Dufeu has indeed added something to Hekekyan Bey's original matter, such as the "Longitude of the Great Pyramid," "the proof that it was never cased," and his own alleged "coffer measures,"—but they are precisely the most flagrantly erroneous parts of the whole book.

PIAZZI SMYTH,
Astronomer Royal for Scotland.

The Moon: her Motions, Aspect, Scenery, and Physical Conditions. By RICHARD A. PROCTOR, B.A. With three Lunar Photographs by Rutherford, and many Plates, Charts, &c. London: Longmans, Green, and Co. 1873. 8vo. 394 pp.

THE admirable works on Astronomy written by Mr. Proctor during the last few years possess many features which make them peculiarly acceptable to this period—a period marked in the history of Science as one in which a general desire is manifested by all classes to obtain accurate, and at the same time popular, knowledge of the universe. These works are eminently popular; they are pleasantly written, the abstruse treatment of difficult subjects is avoided, and the illustrations are abundant and novel. The author aims specially at original treatment; he does not merely give the reader a compilation, after the manner of the generality of popular scientific writers, but he introduces extended descriptions of various phenomena, and frequently supplies information existing only in the memoirs of some learned Society, or not to be found elsewhere. This was no less noticeable in the last book of Mr. Proctor's ("The Sun") which we reviewed in this Journal, as in the present. Between the appearance of these works less than four years have intervened, yet in this time our author has published some three or four works which have taken their place among the popular scientific literature of the day, and which bear upon them the stamp of much earnest and accurate work.

The work before us is divided into six chapters, which treat respectively of—(1) The Moon's Distance, Size, and Mass; (2) The Moon's Motions; (3) The Moon's Changes of Aspect, Rotation, Libration, &c.; (4) Study of the Moon's Surface; (5) Lunar Celestial Phenomena; (6) Condition of the Moon's Surface. These are followed by Tables, and an Index to the Map of the Moon.

The first chapter opens with an account of the view of the Ancients regarding the moon, and the mode of measuring her distance from the earth. From this we learn that Aristarchus of Samos calculated the distance of the moon (by an unknown method) at two millions of stadia, or about 230,000 miles,

while the estimates given by Ptolemy and Alfonso X. approach 235,000 miles. Tycho Brahe calculated the distance as 223,000 miles. The real distance, deduced by Professor Adams from the observations of Breen at the Cape of Good Hope, would appear to be 238,818 miles. We may roughly take the moon's diameter as two-sevenths that of the earth, the moon's surface as twenty-two-sevenths, and her mass at two-ninety-ninths. The surface would be about equal to that of Europe and Africa together, or of North and South America taken together. The earth's disc, as seen from the moon, would appear to be $13\frac{1}{2}$ times larger than the moon appears to us.

The second chapter, which treats of the moon's motions, is one of the most important in the book. It discusses the subject in many points more fully either than Sir G. B. Airy (in his article "Gravitation") or Sir John Herschel (in his "Outlines of Astronomy"). This chapter occupies nearly one-fourth of the entire work, and it is fully illustrated by designs of the author; among others, illustrations of the advance of the perigee and the retreat of the nodes. It is altogether an elaborate exposition of a most difficult subject, which has engaged the attention of the most eminent astronomers and mathematicians of the last century and a half. Our author says, in conclusion, "In the whole history of the researches by which men have endeavoured to master the secrets of Nature, no chapter is more encouraging than that which relates to the interpretation of the lunar motions."

In the account of the study of the moon's surface we have some interesting details concerning the colour of the moon. As to the general results of telescopic observation of the surface, we have to remember the circumstances under which they are made and the power applied. "The highest power yet applied to the moon (a power of about six thousand) brings her, so to speak, to a distance of 40 miles—a distance far too great for objects of moderate size to become visible. Many of my readers have probably seen Mont Blanc from the neighbourhood of Geneva, a distance of about 40 miles. At this distance the proportions of vast snow-covered hills and rocks are dwarfed almost to nothingness, extensive glaciers are quite imperceptible, and any attempt to recognise the presence of living creatures or of their dwellings (with the unaided eye) is utterly useless." . . . Again, as to other difficulties, "We view celestial objects through tubes placed at the bottom of a vast aërial ocean, never at rest through any portion of its depth; and the atmospheric undulations which even the naked eye is able to detect are magnified just in proportion to the power employed. These undulations are the bane of the telescopist. What could be done with telescopes, if it were not for these obstructions to perfect vision, may be gathered from the results of Professor Smyth's observations from the summit of Teneriffe. Raised above the densest

and most disturbed strata, he found the powers of his telescope increased to a marvellous extent. Stars which he had looked for in vain with the same instrument, in Edinburgh, now shone with admirable distinctness and brilliancy. Three delicate striplings of the discs of Jupiter and Saturn, which require in England the powers of the largest telescopes, were clearly seen in the excellent but small telescope he employed in his researches. It is probably not too much to say that even if the Rosse telescope were perfect in defining power, which unfortunately is very far indeed from being the case, yet, on account of atmospheric disturbance, instead of reducing the moon's distance to 40 miles, it would in fact not be really effective enough to reduce that distance to less than 150 miles." Remembering all this, we see that we must receive with great caution observations as to the colour of the moon, the texture of its surface, and so on; the apparently smooth seas or sea-bottoms may in reality be hilly and irregular. The most notable feature of the moon's surface is perhaps the crateriform mountains, which Webb has divided into "walled or bulwarked plains, ring mountains, craters, and saucer-shaped depressions or pits. . . . Copernicus is one of the grandest craters, 56 miles in diameter. It has a central mountain (2400 feet in height, according to Schmidt), two of whose six heads are conspicuous; and a noble ring composed not only of terraces, but distinct heights separated by ravines; the summit, a narrow ridge, not quite insular, rises 11,000 feet above the bottom, the height of Etna, after which Hevel named it. Schmidt gives it nearly 12,800 feet, with a peak of 13,500 feet west, and an inclination in some places of 60°."

Many attempts have been made to determine whether the moon be inhabited or not. Herschel held the former opinion, and many expected that Herschel's or Rosse's great reflector would reveal something of the inhabitants. But in vain. Of course the creatures themselves could not be visible, but large cities might appear; and it has been argued that, as the force of gravity is so much less at the surface of the moon than on the earth, the lunar inhabitants might, without being unwieldy, be much larger than our race of men. "Nor is this argument wholly fanciful. A man of average strength and agility placed on the lunar surface (and supposed to preserve his usual powers under the somewhat inconvenient circumstances in which he would there find himself) could easily spring four or five times his own height, and could lift with ease a mass which, on the earth, would weigh half a ton. Thus it would not only be possible for races of lunarians equal in strength to terrestrial races to erect buildings much larger than those erected by man, but it would be *necessary* to the stability of lunar dwellings that they should be built on a massive and stupendous scale. Further, it would be convenient that the lunarians, by increased dimensions and more solid proportions, should lose a portion of the super-

abundant agility above indicated." It only remains to be mentioned that no object which could possibly be erected by an intelligent being has ever been observed on the moon's surface, even by the use of our most powerful telescopes. Neither, during the two centuries and a half in which the moon has been carefully scrutinised, has any evidence of physical change appeared; all evidence seems, indeed, to show that the moon is "a dead and useless waste of extinct volcanoes."

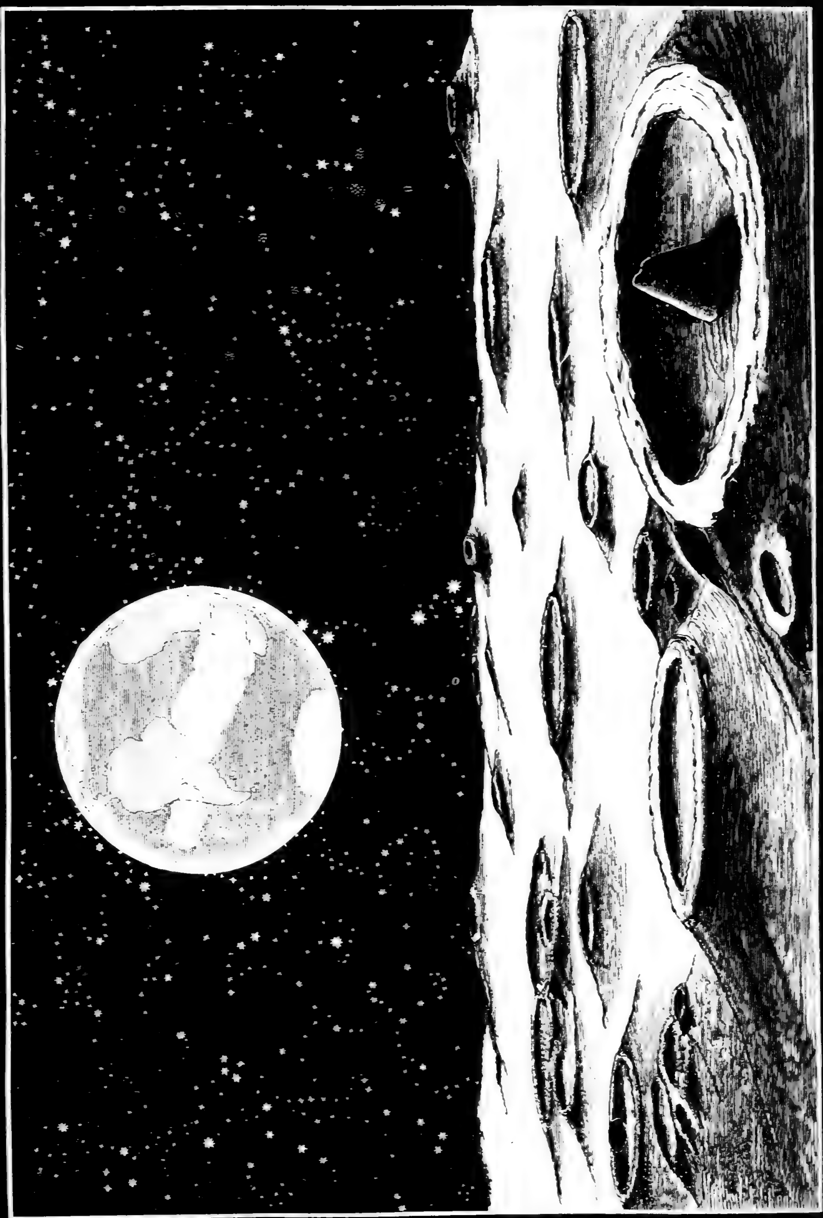
Varieties of colour are noticeable on the moon's surface; some regions appear white, and would be spoken of as *snow-covered* if it were not impossible from the fact that water and air do not exist in the moon. Then there are grey, and greenish, and pale red regions.

An interesting account will be found (pp. 272—282) of the experiments which have been made in order to determine the heat of the moon,—notably the recent experiments of Lord Rosse and M. Marié-Davy. These appear to show that the heat which is received from the moon is mainly radiated, not reflected; that the temperature of the moon's surface is about 500° F.; and that the calorific effect of the full moon is only equal to about one ninety-four-millionth of a degree centigrade. But we must bear in mind that the greater amount of lunar heat which is radiated to the earth is absorbed by the aqueous vapour in our atmosphere.

The fifth chapter discusses, among other things, the possible evidence of a lunar atmosphere. All the known evidence tends to prove that the moon has either no atmosphere at all, or that the atmosphere possesses extreme tenuity. Sir John Herschel, however, and others have admitted the possibility of the existence of an atmosphere on the hemisphere of the moon which is turned away from us, and this theory is based upon the fact that the moon's centre of gravity is nearer to us than her centre of figure.

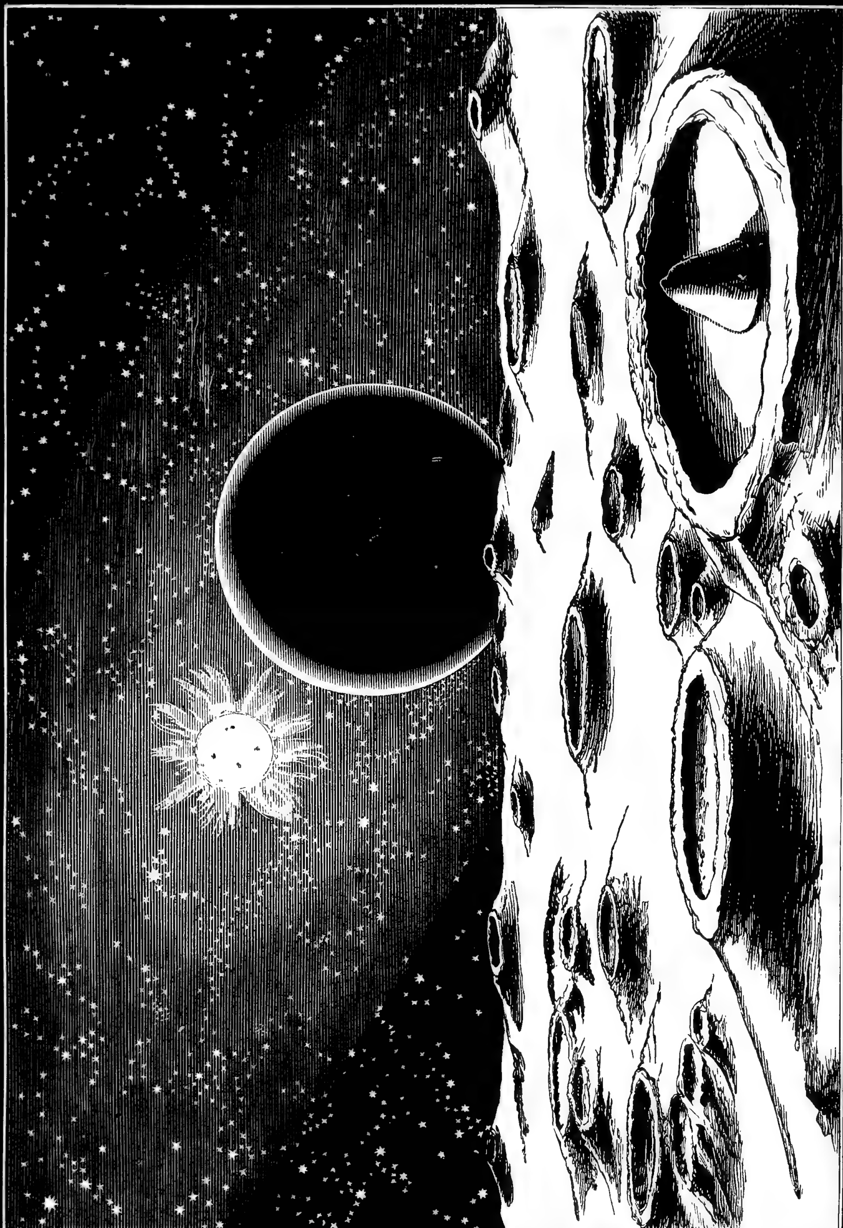
Among the more striking illustrations in the work are two lunar landscapes, drawn by Mr. Proctor. Of course such landscapes can only faintly indicate what we may imagine an observer would see if he were placed on the surface of the moon. Yet as, our author observes, "we know certain facts,—we know that there are striking forms of irregularity; that the shadows must be much darker, as well during the lunar day as during an earthlit lunar night, than on our own earth in sunlight or moonlight; and we know that whatever features of our own landscapes are certainly due to the action of water, in river, rain, or flood, to the action of wind and weather, or to the growth of forms of vegetation with which we are familiar, ought assuredly not to be shown in any lunar landscape. But a multitude of details absolutely necessary for the due presentation of lunar scenery are absolutely unknown to us. . . . In looking at one of these views (Plates XXI. and XXII.) the observer must suppose himself

PLATE XXI.



LUNAR LANDSCAPE—"FULL" EARTH.

PLATE XXII.



LUNAR LANDSCAPE—"NEW" EARTH.

stationed at the summit of some very lofty peak, and that the view shows only a very small portion of what would really be seen under such circumstances in any particular direction. The portion of the sky shown in either picture extends only a few degrees from the horizon, as is manifest from the dimensions of the earth's disc; and thus it is shown that only a few degrees of the horizon are included in the landscape."

This chapter concludes with a very graphic description of what an observer stationed on the summit of the lunar Apennines would have seen on the evening of November 1, 1872. We recommend all our readers to carefully peruse these most eloquent and beautifully descriptive passages. We may only quote one or two paragraphs as examples:—"On all sides, this mighty star-belt spread its out-lying bands of stars, far away on the one hand towards Lyra and Bootes, where on earth we see no traces of milky lustre, and on the other towards the Twins and the clustering glories of Cancer—the 'dark constellation' of the Ancients, but full of telescopic splendours. Most marvellous, too, appeared the great dark gap which lies between the Milky Way and Taurus; here, in the very heart of the richest region of the heavens, with Orion and the Hyades and Pleiades blazing on one side, and on the other the splendid stream laving the feet of the Twins,—there lay a deep black gulf, which seemed like an opening through our star system into starless depths beyond. . . . And now, as hour passed after hour, a series of changes took place in the scene, which were unlike any that are known to our astronomers on earth. The stars passed, indeed, athwart the heavens on a course not differing from that followed by the stars which illumine our skies, but so slowly that in an hour of lunar time they shifted no more than our stars do in about two minutes. And, marvellous to see, the great orb of earth did not partake in this motion. Hour by hour passed away; the stars slowly moved on their course westwards, but they left the earth still suspended as a vast orb of light high above the southern horizon. She changed, indeed, in aspect. The two Americas passed away towards the right, and the broad Pacific was presented to view. Then Asia and Australia appeared on the left, and as they passed onwards the East Indies came centrally upon the disc. Then the whole breadth of Asia could be recognised, but partly lost in the misty light of the northern half, while the blue of the Indian Ocean was conspicuous in the south. And as the hours passed on, Europe and Africa came into view; and our own England, foreshortened and barely visible, near the snow-covered northern region of the disc."

Here, then, we end our brief examination of a work which commends itself both to the general well-informed reader and to the man of Science. The really new matter is by no means inconsiderable, and the work constitutes, we believe, the most

complete monograph on the moon which has yet appeared. We may read every part with a perfect feeling of confidence in the exactness of the matter, remembering that it is at once the work of a mathematician, an astronomer and practical observer, and of an elegant writer.

An Introduction to Physical Measurements. With Appendices on Absolute Electrical Measurement, &c. By Dr. F. KOHLRAUSCH, Professor-in-Ordinary at the Grand Ducal Polytechnic School at Darmstadt, and formerly Professor of Physics at the University of Göttingen. Translated from the second German edition by T. H. WALLER, B.A., B.Sc., and H. R. PROCTER, F.C.S. London: J. and A. Churchill. 1873. 8vo. 244 pp.

SOMEONE (we think Quetelet) has remarked that no science has made any great progress until weight, measure, and number have been introduced into it, in fact, until it has become more or less capable of mathematical treatment. We all know how true this is:—The determination of the mechanical equivalent of heat raised the science to a position which it never before occupied among its brethren; to which result the admirable mathematical deductions of Carnot, Fourier, and, later, of Hirn, Helmholtz, and Clausius have also conduced. Again, Ohm's law, and the various mathematical problems brought to bear upon the subject of electricity by Sir W. Thomson, Clerk Maxwell, and others, have quite revolutionised that science. As an important means to the end indicated above, we welcome the book before us with open hands. It will be invaluable in those physical laboratories which are happily commencing to appear in this country, and which, by the end of the century, will, we trust, have become general in all centres of sound learning.

The author remarks very truly, "that the mere verbal teaching of physical laws is seldom of much use, tending frequently merely to confuse the student, whilst the simple performance of an experiment gives him confidence in himself and in the laws he is investigating." This work on the measurement of physical quantities enables the student the more readily to verify the great laws which obtain in the history of matter and of force.

The introduction treats, in the first place, of "Errors of Observation," and of the influence of error on the final result. And here we meet with advice on a practice which very frequently prevails in the calculation of final results:—"We may here insist upon the fact that it is generally quite inadmissible arbitrarily to exclude from a series of observations some of the number simply because they do not agree with the greater number. The probability of an increased error being

introduced by the irregular numbers will be compensated by the very process of taking the arithmetical mean, for, as single ones among a greater number, they have small influence upon the mean value." This is, no doubt, good advice, but very difficult to follow; if, for example, in a series of twenty observations, seventeen agree very closely, and three are altogether anomalous, or at least differ more widely, the conclusion seems to be almost irresistible that, in these three divergent results, the "personal equation" of the observer has, by some unknown means, been unduly exalted, or some unseen or unremembered error of manipulation has crept into the observation. If made absolutely under the same conditions, of course every determination, however anomalous it may be, and however the series may be prolonged, is subject to equal credence.

The first section is devoted to an account of "Weighing and the Determination of Density." In this, full rules are given for the adjustment and testing of a balance and of the weights; also various modes of weighing, and the determination of the density of solids, liquids, and gases. This is followed by measurements relating to heat; the calibration of a thermometer, and determination of its fixed points; the various methods of calorimetry, &c.

The determination of the modulus of elasticity of a body is discussed under various forms, as by stretching, and by the complex and somewhat unusual method by longitudinal vibrations, in which the necessary factors are (α) the length of the wire, (β) its specific gravity, (γ) the acceleration by gravity, (δ) the number of longitudinal vibrations per second. The time of vibration is determined by a tuning-fork of known pitch, and the longitudinal vibrations are produced in the usual manner, by rubbing the rod with woollen cloth sprinkled with resin. The determination of the modulus by bending a rod, and by swinging under torsion, is also described.

The optical measurements include various determinations connected with spectroscopy, the wave-length of a ray of light, the focal length of a lens, the magnifying power of optical instruments, the operations of saccharimetry by polarised light. Finally, a number of magnetic and electrical measurements are described in detail.

The tables at the end of the volume will be found of considerable service; here we find, among others, the density of certain gases given to seven places of decimals, the density of water to five places of decimals between 0° and 30° C., the expansion of water, the density of air at various temperatures, and the capillary depression of mercury in a glass tube of from 1 to 10 millimetres diameter. Table 19 gives the lines of flame-spectra of the most important light metals, according to the scale of Bunsen and Kirchhoff, in which the slit is considered to have the breadth of one division, while the sodium line is taken

at 50. Under these conditions, the colours of the spectrum are approximately as follows:—Red to 48, yellow to 52, green to 80, blue to 120, and violet beyond.

A Dictionary of Terms Used in Architecture, Building, Engineering, Mining, Metallurgy, Archæology, the Fine Arts, &c. By John Weale. Fourth Edition, with Numerous Additions. Edited by Robert Hunt, F.R.S. London: Lockwood and Co. 1873.

When a work has so rooted itself in our literature as to reach a fourth edition, the business of a reviewer becomes tolerably simple. Indeed the public has long ago formed its own opinion as to the merits of the work; and the very existence of the successive editions bespeaks its value more clearly than any favourable expressions that may fall from the reviewer's pen.

The rapid growth of modern science, and the consequent extension of scientific terms to the various branches of industry and art, renders it more than ever necessary that we should constantly have at hand some trustworthy work of reference on technical nomenclature. It is difficult to point to a handier book for this purpose than Weale's well-known Dictionary; it is comprehensive, though small, and is, indeed, just the book which one may confidently consult when seeking the interpretation of some obscure word in the language of our industrial arts.

The present edition of Weale's Dictionary has had the benefit of careful revision by Mr. Robert Hunt, whose long experience with kindred literature leads him to know exactly the kind of matter which the public seeks and expects to find in a work of this character. The editor has judiciously adopted, in preparing this edition, a more systematic arrangement of the matter; and, by omitting the biographical sketches which appeared in the earlier editions, has contrived to squeeze in a goodly amount of new matter without increasing the bulk of the book.

A rapid glance down the columns of this Dictionary is sufficient to show that even the best informed amongst us may often turn with profit to its technical definitions. Take, as accidental examples, the first and last words in this Dictionary;—surely it is not everyone who, if suddenly called upon, could give a satisfactory definition of either *Aam* or *Zyghar*.

Celestial Objects of Common Telescopes. By the Rev. T. W. Webb, M.A., F.R.A.S., Vicar of Hardwick, Herefordshire. London: Longmans, Green, and Co. Third Edition, Revised and Enlarged.

It is gratifying to learn that such a book as this has reached a third edition. It is not a library book, is by no means likely to

be read by Mudie's subscribers, nor purchased by anybody for the mere sake of perusal. It is simply a Guide-Book for travellers in the heavens, one intended and well adapted to the wants of those who have decided to devote some portion of their surplus means to the noblest of human pursuits—the direct study of Nature. We may therefore regard the demand for such a book as a measure of independent astronomical research. We use this word "research" deliberately, and apply it to the humblest efforts of the humblest possessor of the smallest of telescopes who uses such an instrument, or even the naked eye, for the purpose of obtaining knowledge direct from the heavens. We have no sympathy with those scientific prigs who pretend to despise amateur astronomers, and would lead the docile readers of Quarterly Reviews, &c., to believe that a "broad basis of scientific culture" is the exclusive prerogative of University professors and officials. All who have followed the recent progress of Astronomy in this country must be struck with the great amount of scientific work of the highest order that has been done by pure amateurs, by men who have begun with small telescopes and unpretending efforts, and have been led on, by the fascination of the subject, to purchase more and more perfect instruments and aim at higher and higher work, until—in those cases where wealth has accompanied scientific enthusiasm—they have found themselves the proprietors of observatories in which they have made some of the most important of modern astronomical discoveries. The existence of such a body of able amateur astronomers as constitute a large proportion of the Fellows of the Royal Astronomical Society is alike honourable to the nation and advantageous to Science, and we welcome the third edition of Mr. Webb's Handbook as a valuable aid and incitement to valuable and disinterested scientific enthusiasm.

The Depths of the Sea. An Account of the General Results of the Dredging Cruises of H.M.SS. *Porcupine* and *Lightning* during the Summers of 1868, 1869, and 1870, under the Scientific Direction of Dr. Carpenter, F.R.S., J. Gwyn Jeffreys, F.R.S., and Dr. Wyville Thompson, F.R.S. By C. Wyville Thompson, LL.D., F.R.SS. L. and E., F.L.S., &c., Regius Professor of Natural History in the University of Edinburgh, and Director of the Civilian Scientific Staff of the *Challenger* Expedition. London: Macmillan and Co.

Science is undoubtedly cosmopolitan; nevertheless the business of scientific research may be materially promoted by a certain amount of international division of labour. If "Britannia rules the waves" she ought to take the lead in studying all that lies beneath and about them. There are solid as well as sentimental reasons for this. We have a huge navy. Our finest ships are

liable to rot if left lying idle in dock or harbour, and the best of sailors are subject to analogous corruption unless provided with some kind of active occupation. A genuine sailor has a huge contempt for useless inactivity and lubberly land-lounging, but in these "piping times of peace" he has no small difficulty in finding some plausible pretext for a cruise. Pirates are practically extinct; there is nothing to be done in chasing them; the only approach to old-fashioned naval occupation now remaining open to him is the weary blockading of the pestiferous mouths of swampy African rivers for the meagre chance of occasionally capturing a slaver.

The *Times* newspaper has recently taken up the subject of Arctic Exploration, and would have us give up all further attempts to solve the polar problems, because the private and imperfectly-organised expedition of the *Polaris* has compelled some sailors and Esquimaux to suffer the hardship of wintering on a drifting ice-floe. If sailors were helpless babes and the *Times* were the national wet-nurse for marine infants, this tender solicitude would be emphatically proper and dutiful, but, as it is, the opinions of the quarter-deck and forecastle are far better guides for outsiders, like ourselves, than those of Printing-House Square. If responsible officers and sober men, who have already had some practical experience of arctic hardships and dangers, are willing and eager to incur them again, and if full-grown civilian naturalists are equally urgent in their desire to share the sailor's perils, it would be little short of insult if the nation at large were to accept the conclusions of the *Times*, and refuse to enter upon further arctic exploration merely on the pretext of maternal tenderness. If the whole truth could be told, we should probably learn that the risks of physical suffering which our sailors encounter in the streets of Valetta, Naples, Genoa, Marseilles, and other Mediterranean ports, while their ships are lying idly in harbour, are quite as great as those to which they are exposed when threading their way between Greenland icebergs.

If any statesman desires to learn how the spare ships and men of the British Navy should be occupied during times of peace, let him send at once to Bedford Street for a copy of Dr. Thompson's "Depths of the Sea;" let him read it thoughtfully, and compare it with the log-book of the ordinary ships of war which we are compelled, at great expense, to maintain in sailing condition for mere preparation sake. He will see, by the continual reference to the hearty co-operation and valuable aid of the naval officers, how readily and aptly the sailor takes to scientific work; and when he reflects on the fact that warfare is becoming more and more a struggle of scientific engineering, the importance of the prevalence of scientific habits of mind among naval officers must be obvious. If he has any old-fashioned patriotism, the perusal of this luxurious volume must stir up a healthy British pride in the truly glorious conquests of the *Porcupine* and the

Lightning during the summers of 1868, '69, and '70, and show him how vast are the conquests that may yet be made in the new and higher career of honour which is opened up for the British flag by such expeditions.

Most of our readers know already, by the reports that have been published from time to time in the scientific and other journals, what was done during these well-spent summers; and we recommend all to revel, as we have done, in the luxury of travelling again over Dr. Thompson's connected and beautifully illustrated narrative of the whole of the proceedings. As a profoundly valuable contribution to science, as a literary effort of high order, and as an elegantly artistic volume, this book is worthy of the warmest praise. It is indeed fortunate that the results of such important expeditions should be recorded by so able a writer as Dr. Thompson, and that such a writer should find such spirited publishers as Messrs. Macmillan and Co.

The simplicity of style and clearness of description are very high merits. The whole book is readable by any man or woman of ordinary liberal education, and this great merit is attained without any sacrifice of scientific technicality or precision. We sincerely hope that the privilege of reading the original record of such important scientific work will be fully and popularly appreciated, as it is not often that researches which have had so important an influence on some of the foundations of cosmical science are thus easily accessible.

The most important philosophical results of the expeditions are summed up in the concluding essay on the "Continuity of the Chalk," wherein the author states his reasons for concluding that, in spite of the myriads or millions of centuries that must have elapsed since the deposit of the chalk which lies beneath our feet here in London, there has been no chasm of time, no interregnum of deposit between this ancient and the actual but somewhat modified chalk formation now proceeding at the bottom of the Atlantic.

Admitting to a certain extent the justice of the objections made by Sir Roderick Murchison and Sir Charles Lyell, to his early expression that "we are still living in the cretaceous epoch," on account of the indefinite sense of the terms "geological epoch" and "geological period," Dr. Thompson shows good reason for maintaining the conclusion which these words were intended to express, viz., that "the various groups of fossils characterising the tertiary beds of Europe and North America represent the constantly altering fauna of the shallower portion of an ocean whose depths are still occupied by a deposit which has been accumulating continuously from the period of the pre-tertiary chalk, and which perpetuates with much modification the pre-tertiary chalk fauna;" or otherwise, that "we must regard the tertiaries as the deposits formed and exposed by depressions and upheavals of the cretaceous sea: of a sea

which, with many changes of condition produced by the same oscillations which alternately exposed and submerged the tertiaries, existed continuously, depositing conformable beds of chalk-mud from the period of the ancient chalk."

The important bearing of these conclusions upon the very foundations of geology are obvious enough, and are rendered more strikingly so when expressed in the still more pointed language of Professor Huxley, "that the modern chalk is not only the lineal descendant, so to speak, of the ancient chalk, but that it remains, so to speak, in possession of the ancestral estate; and that from the cretaceous period (if not much earlier) to the present day the deep sea has covered a large part of what is now the area of the Atlantic. But if *Globigerina* and *Terebratula*, *caput serpentis* and *Beryx*, not to mention other forms of animals and plants, thus bridge over the interval between the present and the mezozoic periods, is it possible that the majority of other living things underwent a sea-change into something new and strange all at once?"

Such suggestions are almost revolutionary, and if confirmed they will cruelly spoil the orthodox lecture-room diagrams of the geological ladder and the common stratigraphical descriptions of superposition of rocks in the order of time. All the symmetry of geological chronology will be destroyed if the cretaceous system is to run up through the *eocene*, the *miocene*, the *pliocene*, the *pleistocene*, and the *recent;* and we must cease to call these by the name of "periods," as they may merely indicate localities or variations of sea-depth. If the chronological conclusions based upon the stratigraphical arrangement of these later rocks are thus shaken, may not something analogous have occurred when the lower rocks were forming? May there not be other cases where depths of ocean have been mistaken for depths of time? or, in other words, may not many geological formations hitherto described as deposited successively have actually been proceeding, to some extent, simultaneously?

Thus, again, we are presented with an important phase of the great question of evolution. If the creatures now living in the great depths of the Atlantic can be proved to be the true descendants of those of our chalk cliffs, and their line of ancestry can be traced continuously, we have command of a vast period of time under which to study the laws of modification of varieties, of species, and even perhaps of genera.

But these are problems of vast magnitude, which the cruises of the *Lightning* and the *Porcupine* have only opened or suggested, and which we may hope that the *Challenger* will open yet wider; their complete solution will demand an amount of further research proportionate to their magnitude and great philosophical importance.

We are satisfied that every reader of "The Depths of the Sea" who is earnestly interested in the progress of Science and

the true honour of his country will share our comfortable satisfaction in knowing that Dr. Wyville Thompson is the Director of the Civilian Staff of the *Challenger* Expedition; and our conviction that when future historians recite the sea-battles that have been gloriously fought under the British flag, and the names of the ships that have carried it to victory, those of the *Lightning*, the *Porcupine*, and the *Challenger* will take leading rank in the list; and also in our hope that the British Navy is entering upon a new era of higher and better conquests than those which have hitherto fed the pride of the nation.

Critiques and Addresses. By THOMAS HENRY HUXLEY, LL.D., F.R.S. London: Macmillan and Co. 1873.

THIS is a collection of Essays like the "Lay Sermons, which, as Dr. Huxley says, "indicate the high-water mark of the various tides of occupation by which I have been carried along since the beginning of the year 1870. They include the following subjects:—"Administrative Nihilism." "The School Boards: What they Can Do and What they May Do." "Medical Education." "Yeast." "The Formation of Coal." "Coral and Coal Reefs." "The Methods and Results of Ethnology." "Some Fixed Points in British Ethnology." "Palæontology and the Doctrine of Evolution." "Biogenesis and Abiogenesis." "Mr. Darwin's Critics." "The Genealogy of Animals." "Bishop Berkeley on the Metaphysics of Sensation." These titles sufficiently indicate the range of subjects, and the impossibility of including within our limits anything like an analysis or discussion of the contents of this volume. The subjects and their treatment are strikingly characteristic of the noble breadth of Dr. Huxley's attainments and philosophy.

The vast accumulations of modern knowledge have rendered a division of labour among the experts in Science a matter of absolute necessity. We have undoubtedly gained great advantages by one man devoting his life to mathematics, another to metallurgy, a third to organic chemistry, a fourth to comparative anatomy, &c.; it has enabled either of these to learn all that has been done in his particular department, and thus to start fairly upon the path of original investigation. This arrangement is, however, not unaccompanied with disadvantages, some of them rather serious. Among these is the common practice of assuming that Physics, Chemistry, Physiology, Political Economy, Metaphysics, Moral Philosophy, &c., are subjects that have actual separate existences in the scheme of Nature, rather than regarding them in their true aspect as artificial subdivisions of the one and only science of Universal Natural Law, and remembering that such divisions have been made for the accommodation of human weakness. There

are but few of our modern teachers of Science that are able fairly to divest themselves of the cramping influences of this patchwork view of Nature. Amongst these few Dr. Huxley stands out with leading prominence. His special study having been that of Biology, which is placed, so to speak, midway between the physical and moral subdivisions of Natural Science, he stands upon a middle eminence, from which he can best survey the equally surrounding area of human knowledge. With an intellectual vision of unusually great penetrating power, and a moral nature of well-balanced proclivities, he is thus able to present to his readers, with remarkable vividness and impartial truthfulness, a picture of the great panorama thus placed at his feet. The "Critiques and Addresses" is a series of these pictures, not avowedly connected and yet not altogether detached. They are all painted with remarkable artistic power, and more or less characterised by a catholicity of treatment which obliterates the artificial boundaries that have been mischievously set up between, physical, physiological, moral, and theological science. The powerful essay on "Mr. Darwin's Critics" is a fine example of this. It is positively refreshing to be able to travel in the midst of a purely scientific atmosphere over a fertile region of thought which is usually rendered pestiferous by the miasma of theological bigotry, and to leave it, as we may after wandering under Professor Huxley's guidance, with a healthily invigorated intellect.

In the first two papers we have political subjects treated in like manner, without at all descending to "the region in which Tories, Whigs, and Radicals 'delight to bark and bite.'"

In the course of his enquiry into the limits of Government functions, Prof. Huxley comes in collision with the conclusions of his friend and fellow-worker—we might almost say twin-brother in Science—Mr. Herbert Spencer, and the consequent combat is conducted with the utmost vigour on both sides, accompanied with a polished courtesy suggestive of a courtly fencing bout between the most chivalric of antagonists. Accepting Mr. Spencer's parallel, Prof. Huxley contends that "the vascular system, or apparatus for distributing commodities in the animal organism, is eminently under the control of the cerebro-spinal nervous centres—a fact which, unless I am again mistaken, is contrary to one of Mr. Spencer's fundamental assumptions. In the animal organism Government does meddle with trade, and even goes so far as to tamper with the currency." We will not venture to step between such combatants, or even to record the "hits" on either side, but merely state in the meantime that few can follow this controversy on a two-sided subject without profiting considerably by seeing both sides so well displayed.

While the subjects usually supposed to belong rather to the provinces of ornamental or controversial literature than to science

are thus treated in accordance with strict inductive philosophy, without losing anything of literary refinement or readable simplicity, the other subjects, commonly supposed to be more strictly scientific, are by no means presented in the bare skeleton style of pedantic disquisition which is popularly supposed to be the truly scientific style, but are exhibited to the reader in their truly natural forms, enveloped in the rounded and tinted integuments of a polished literature, which appeals to the emotional—the poetic faculties of the reader, as well as to the purely intellectual powers. In reading these we seem to be listening to a voice that comes genially across a dinner table rather than coldly from a professorial chair.

With these characteristics, we have no doubt this collection of "Critiques and Addresses" will be as popular and usefully influential as the "Lay Sermons" that were published three years ago.

Reprint of Papers on Electro-Statics and Magnetism. By Sir WILLIAM THOMSON, D.C.L., LL.D., F.R.S., F.R.S.E., Fellow of St. Peter's College, Cambridge, and Professor of Natural Philosophy in the University of Glasgow. London: Macmillan and Co. 1872.

A Treatise on Electricity and Magnetism. By JAMES CLERK MAXWELL, M.A., LL.D. Edin., F.R.SS. London and Edinburgh, Honorary Fellow of Trinity College, and Professor of Experimental Physics in the University of Cambridge. 2 vols. (Clarendon Press Series.) London: Macmillan and Co. 1873.

THOSE amongst our readers who are acquainted with periodical scientific literature will find in Sir W. Thomson's "Reprints" much they have already studied and much that will be new to them. But the first idea one obtains from the volume of nearly six hundred pages is the immense amount of unpublished work implied. It is a characteristic of physical science memoirs, and a reason for their scarcity, that they represent not only so much work done upon paper, but long and tedious labours in the laboratory, endless disappointments from having taken the wrong road, and turnings back which have certainly served the purpose of rendering surer the way for future travellers, but wearying in the extreme to the explorer. Another surprise is the practical results that may accrue from purely mathematical deductions, but of which, upon closer inspection, the secret turns out to be the logical and comprehensive argument involved. Such views are encouraging, especially in the science of electricity. We were fast growing into the habit of looking to Germany for our mathematical investigation of electro-statical and electro-magnetical science, and this not because these papers were unknown,

but because they were too scattered for immediate reference. Great good has been done by their collection, for they are, to use a German expression, "science-making" in their nature.

We will note in the order of the papers as nearly as possible the chief points of Sir William Thomson's work. The first paper deals with the exceedingly difficult problem of the uniform motion of heat in homogeneous solid bodies and its connection with the mathematical theory of electricity. Before this paper there were prominently current two questions—one resulting from Fourier's investigation, by which it appeared that the conduction of heat is proportional to the rate of variation of temperature at point to point of the conductor; the other related to the distribution of electricity on conductors, and included the assumption that the electrical particles exerted mutual forces varying as the square of the distance. One implies a flow, the other instantaneous action. Yet these questions, so contrary in the experimental methods of investigation, were proved by Thomson to be mathematically one. Substitute in Fourier's formulæ electrical surface for heat surface, electric potential for temperature, and at once they are fitted to the use of the electrician.

And very much more than this collation of scientific principle, the highest of generalisation do we owe to the present occupier of the Professorial Chair at Glasgow. To Sir William Thomson are due the embodiment of Faraday's idea of continuous action in mathematical formulæ, the method of electrical images, and of electrical inversion. The practical applications of his mathematical theories are too well known to need detailing here, and the list is too long, for it includes researches into atmospheric electricity, the construction of electrometers, of galvanometers, telegraphic instruments, and experimental researches into all branches of electricity.

In Professor Thomson's book we really find the germ of much important work of our second author; for in one of Thomson's earliest papers—that on the attractions of conducting and non-conducting electrified bodies, published in 1843—we find the speculation commenced that Professor Maxwell has since made famous, the complete mathematical rendering of Faraday's physical lines of force. Then passing on to the consideration of vortex motion, we have Professor Maxwell again in the same field. But these developments are essentially original works, for so narrow and yet so liberal must be the reasoning that every step is an *opus magnus*. Here the resemblance between the two books ceases. Professor Maxwell's book, while it affords instruction to the student, really includes the most important of the higher theories of electrical science, the first issue being the treatment of Faraday's lines of force. How he has attempted this we must let our author speak for himself, The general complexion, he says, of the treatise differs considerably from

that of several excellent electrical works, published, most of them, in Germany; and it may appear that scant justice is done to the speculations of several eminent electricians and mathematicians. One reason of this is, that before I began the study of electricity, I resolved to read no mathematics on the subject till I had first read through Faraday's "Experimental Researches on Electricity." I was aware that there was supposed to be a difference between Faraday's way of conceiving phenomena and that of the mathematicians, so that neither he nor they were satisfied with each other's language. I had also the conviction that this discrepancy did not arise from either party being wrong. I was first convinced of this by Sir William Thomson, to whose advice and assistance, as well as to his published papers, I owe most of what I have learned on the subject.

As I proceeded with the study of Faraday, I perceived that his method of conceiving the phenomena was also a mathematical one, though not exhibited in the conventional form of mathematical symbols. I also found that these methods were capable of being expressed in the ordinary mathematical forms, and thus compared with those of the professed mathematicians.

For instance, Faraday, in his mind's eye, saw lines of force traversing all space where the mathematicians saw centres of force attracting at a distance. Faraday saw a medium where they saw nothing but distance. Faraday sought the seat of the phenomena in real actions going on in the medium; they were satisfied that they had found it in a power of action at a distance impressed on the electric fluids."

The portions of this work relating to the construction of galvanometers and other electrical instruments show the author to be as much at home in the workshop as the scientific world knows him to be in his study.

The Year-Book of Facts in Science and Art. By JOHN TIMBS. London: Lockwood and Co. 1873.

YEAR after year Mr. Timbs continues with praiseworthy industry to collect all kinds of scientific scraps, and to piece them together in the shape of these handy little volumes. The year-book for 1872, like its predecessors, contains an interesting collection of extracts which are generally well chosen; and though one may have met with most of the paragraphs elsewhere, it is convenient to have them at hand in a form easily available for reference. It is, however, to be wished that the compiler would extend his labours to the original sources of information offered by the Proceedings of our learned Societies; for the reader who consults his annals often craves for some higher authority than the daily and weekly journals which form the great repository whence Mr. Timbs draws most of his information. Still we have to thank the editor for taking the trouble to preserve in these annual

records many useful paragraphs which would otherwise be soon lost in our ephemeral literature.

Mr. Timbs gives us, as usual, an obituary list of persons eminent in science, art, and literature, who have died during the year. A popular account is then given of most of the inventions in the mechanical and useful arts, and the principal discoveries in physical and natural science. The doings of the British Association at the Brighton Meeting come in for a full share of notice; and, finally, we have a table devoted to a summary of meteorological observations in 1872.

If Mr. Timbs's year-books cannot pretend to fully chronicle the progress of science, or to equal in completeness some of the German Jahrbücher, it must nevertheless be admitted that they are extremely handy books of reference, and may be fairly and favourably compared with the little yellow-covered volumes which we are in the habit of receiving from Paris—Figuier's "L'Année Scientifique et Industrielle."

Report on the Filtration of River Waters, for the Supply of Cities, as practised in Europe, made to the Board of Water Commissioners of the City of St. Louis. By JAMES P. KIRKWOOD, C.E. New York: D. Van Nostrand. London: Trübner and Co. 1869.

THE supply of wholesome water for domestic purposes is one of the most important questions of the present day, and in collecting and publishing the most recent experience as carried out in different towns of Europe, the Board of Water Commissioners of St. Louis have done a great public service; and from a perusal of the volume now before us we have no hesitation in stating that in the selection of Mr. J. P. Kirkwood for that duty their confidence has been in no way misplaced. The principal portion of this Report is devoted to the different forms of filtering beds used at the water-works in the principal towns of England, France, Germany, and Italy, whilst in the Appendices descriptions are given of the duty performed by the London pumping engines and their boilers, as an important subject in connection with the general question. The book is also copiously illustrated with well-executed engravings, which adds considerably to its value as a standard work of reference.

The amount of silt carried by rivers from which a water supply is drawn is, of course, an important question, as well as the nature of the districts through which they run, and the amount of contamination their waters receive from surface drainage off highly-manured fields and from the drainage of manufacturing districts. In a few places what is called the natural filter is in successful use. Where artificial filters are required, as is most generally the case, the materials used for their construction are

sand, gravel, and broken stone or shingle, the depth of the whole varying from 5 to $6\frac{1}{2}$ feet; a layer of cells has sometimes been used placed between the stratum of gravel, but this is not found essential, and is now generally omitted. In some cases this filter bed rests upon a foundation of puddled clay or concrete, where there is a loose and porous sub-stratum beneath. Each filter bed, at short intervals varying with the condition of the water, must have the deposit which accumulates on the surface of the sand cleared off or removed, and while any one is undergoing this process, the other remaining filters must be competent to deliver the required supply without overstraining their functions. If, then, there are six filters, five of them must be competent to the full duties of the service, and if eight filters, seven of them must be competent to this duty, on the supposition always that not more than one filter will at any time be off duty. Should the circumstances in effect render two unserviceable, the remainder must have area enough to meet the requirements of the case.

Space will not admit of our entering further into detail relative to the peculiarities of filter beds in different localities and under varying circumstances, but these will be found carefully considered in the pages of the Report itself, to which we must refer our readers if they desire more reliable information upon the subject.

PROGRESS IN SCIENCE.

MINING.

Gold Mining in the Colony of Victoria continues in a healthy state of activity, judging, at least, from the volume of "Mineral Statistics for 1872," which has been prepared as usual by Mr. Brough Smyth, and recently issued under the authority of the Minister of Mines at Melbourne. These statistics do not pretend to offer anything more than approximate figures, for the returns are given voluntarily, and there is therefore no means of obtaining them from those miners who are not sufficiently acute to see that the collection of such statistics must even be of the greatest benefit to the mining community at large, and as individual statements are merged in totals that can never be prejudicial to the interests of individual miners. But, though the Victoria returns may not state precisely the quantity of gold raised during the year, they evidently merit a considerable measure of confidence. According to the estimates made by the several mining registrars, there was obtained in 1872 about 1,331,377 ozs. of gold. The returns furnished by the Commissioner of Trade and Customs give 1,160,554 ozs. 19 dwts. as the quantity of gold exported during the year: whilst the Melbourne Branch of the Royal Mint has received 121,965 ozs. 17 dwts. Again, the returns made by the managers of the several banks showed that they purchased during the year 1,218,094 ozs. 9 dwts.

In studying the detailed statistics and comparing them with those of the previous year, it is satisfactory to note an increase in the average yield of gold from quartz, and also an increase in the quantity of stuff treated; there is, however, a slight falling off in the quartz tailings, mullock, &c., worked in the past year. Mr. B. Smyth calls attention to a cheap and simple, though slow, process for separating gold from pyrites, tailings, and mullock. By stacking such materials with a due proportion of small coal, gum leaves, and other vegetable matter, the gold would be slowly set free, and could then be readily collected. Appended to the volume of statistics is a report by Mr. J. Cosmo Newberry on the work done during the past year in the Melbourne laboratory.

The magnitude and value of the mineral resources of our Australian Colonies are well represented in the Australian Annexe to this year's International Exhibition. South Australia is especially prominent, exhibiting some splendid examples of copper and iron ores; the former including fine samples of native copper, red oxide, copper pyrites, purple ore, malachite, and atacamite from the mines on Yorke's Peninsula; whilst among the iron ores are some noble specimens of magnetite, hæmatite, and limonite. The South Australian copper industry is also represented by a metallurgical series from the Wallaroo smelting works. Bismuth ore from the Balhannah mine, and ingots of metallic bismuth smelted from this ore, are also exhibited; whilst the gold-fields, of which one has not yet heard much, are represented by some valuable specimens. The interest of this collection is greatly enhanced by the publication of an excellent catalogue.

New South Wales, in addition to its fine samples of coal, exhibits some splendid specimens of tin ore, illustrating the recent discoveries which were duly chronicled in these columns. Victoria sends some capital collections arranged by Mr. Brough Smyth, and well exhibiting the capabilities of the Colony. In the Queensland Annexe, an excellently arranged department, we note especially the fine specimens of precious opal recently discovered. This opal appears to occur in the form of a thin layer in fissures of ironstone nodules, but though of extremely fine colour with no lack of fire, it seems too thin to be of much value to the jeweller. We have lately seen some samples of opal, discovered under different conditions in the adjacent colony of New South Wales, but these are not yet represented in the Exhibition.

Some interesting information on the mineral resources of Upper Burmah is given by Captain G. A. Strover in a recently-issued official report. It appears that iron ore abounds in the Shan States, and that a manufactory on very crude principles is at work at Pohpah Toung. A rich hæmatite has been found abundantly to the west of Sagaing, and the requisite plant for establishing large works for smelting this ore is to be sent over from this country. Coal is known to occur in several localities in Burmah; some being, it is true, of only inferior quality, resembling lignite, whilst other varieties are said to equal the best English coal.

Several interesting papers bearing on Cornish mining were communicated to the Institution of Mechanical Engineers at its recent meeting in Penzance. Among them we may refer to a paper by Mr. J. H. Collins, "On the Mining District of Cornwall and West Devon," in which the writer gave a general description of the geological features of the district, the mode of occurrence of its mineral veins or lodes, and the methods of Cornish mining. In a paper "On Machinery for Dressing Tin and Copper Ores," Mr. H. T. Fergusson described in detail the different forms of tin stamps, and explained the advantages of Husband's patent pneumatic stamps; the author also noticed the chief improvements in the dressing of copper ores, including Borlase's buddle, Dingley's pulveriser, and Oxland and Hocking's patent calciner. A description of the tin steam works in Restronguet Creek, near Truro, was presented by Mr. C. D. Taylor, of Devoran. These works yielded large quantities of tin at the end of the last century, and after various vicissitudes have again been opened up by Messrs. Taylor. The Institution visited these works among other places of interest in the county.

Coal-cutting machinery has undergone considerable improvements at the hands of Messrs. Simpson and Hurd. After introducing many modifications, they have succeeded in producing a very superior machine. A couple of these machines—the first two which had been completed by the makers, Messrs. Matther and Platt, of Manchester—were recently exhibited at Wigan to the members of the South Staffordshire and East Worcestershire Institution of Mining Engineers. Some lithographs of the machine have been published in the *Colliery Guardian* (Aug. 8).

It is reported that recent explorations in the Hundred of Wirral in Cheshire, between the Dee and the Mersey, have led to the opinion that several seams of good coal exist within this area. A bed of coal, 11 feet thick, is, indeed, said to have been discovered on the Trelawney Estate at Songhall Massie.

We also hear of the discovery of a large lode of hæmatitic iron ore in North Devon, and of a rich silver-lead lode at Poolvash in the Isle of Man.

METALLURGY.

For the first time in the history of the Iron and Steel Institute, it has held a Continental Congress. Liège, on the Meuse, surrounded by some of the great iron-producing districts of Belgium, was the scene of its late session. In delivering the presidential address, Mr. I. Lowthian Bell gracefully alluded to the amicable relations between Belgium and Great Britain, and referred to the great value of the Belgian coal-fields, the exceptional character of their coal seams, and the ingenuity of her mining engineers in developing these resources under adverse natural conditions. It appears that during the past year the output of the Belgian collieries was about 14 millions of tons, and that between 5 and 6 millions were exported. The exports also included nearly 800,000 tons of hæmatitic iron ore, though iron ores are at the same time largely imported into Belgium. The president reminded our neighbours that the development of their iron trade was in large measure due to one of our countrymen, John Cockerill, who erected the first coke blast-furnace in Belgium, and thus laid the foundation of the great iron works of Seraing. Mr. Bell had a good word in favour of the *Ecoles des Mines* of Liège, Mons, and Charleroi, and for the kindred schools in other parts of Belgium. Nor did the excellent technical journal which issues from Liège—*Cuyper's Revue Universelle des Mines*—pass without its due measure of praise. Finally, the

president took occasion to compare the iron industry of Belgium, France, and Germany with that of our own country.

In a very readable paper communicated to the Institute, M. Julien Deby traced the rise and progress of the iron and steel industries of Belgium. Going back beyond the ken of history, he said that archæological discoveries of quite recent date, still unpublished, seemed to indicate that at the period of the Roman Invasion iron had already been made in Belgium though unknown to the inhabitants of the British Isles. M. Piot has found in Brabant and elsewhere vast heaps of cinder, associated with stone arrow-heads and fragments of coarse pottery, the relics of a non-historic iron-working folk. In connection with the recent progress of the Belgian iron industry it may be stated that the first Danks's rotary puddling furnace was erected a few months back at the works of the Société Anonymé of Sclessin. As to the Belgian steel trade, which is comparatively of recent date, it appears that both Bessemer's and Siemens's processes are largely used, and that in 1872 as much as 15,284 tons of steel were made in the province of Liège alone.

A paper "On the Economical Preparation of Iron for the Danks's Puddling Furnace" was read before the Institute by Mr. C. Wood, of Middlesbrough. One of the difficulties of Danks's system is the mode of melting the pig-iron; if, as is commonly the case, the pigs are broken in halves and thrown direct into the rotary furnace, a long time is occupied in melting them, and during this time the furnace cannot be rotated, as any motion would evidently tend to knock the heavy bars against the lining of the converter, and thus cause injury. To obviate this inconvenience, it has been proposed to melt the iron in a cupola before running it into the rotator. But as this is an expensive method, Mr. Wood suggests that the pig-iron should be granulated by aid of the simple machine which he has successfully used for granulating blast-furnace slag. The revolving furnace is charged with the granulated pig-iron, and set in motion at once.

It is well known that for some time past, Dr. C. W. Siemens, F.R.S., has been actively engaged in developing his processes for the direct conversion of iron ores into wrought-iron and steel. His results were laid before the Chemical Society last spring in a valuable lecture "On Smelting Iron and Steel," which has been published in the July number of the Society's journal. It is unnecessary to offer an abstract of this lecture, as the metallurgical reader can so readily refer to the original, where he will have the benefit of consulting the accompanying illustrations.

Messrs. Gerhard and Caddich, of the Brierley Foundry, Bradley, have lately been turning out blooms of finished iron made direct from the ore by a new process. Ground hæmatite is mixed with fluxing and reducing agents in the form of lime and pitch, and the mixture baked in a coke oven. A furnace is charged with this preparation, and it is said that in half an hour the iron is turned out ready for the helve or the squeezers.

As all questions relating to the economy of fuel are of first importance to the metallurgist, we readily call attention to an excellent paper by Mr. Emerson Bainbridge, "On Coppée's Patent Coke Ovens, and the extent to which their Waste Gases can be Utilised," a paper recently published, with numerous illustrations, in the "Transactions of the North of England Institute of Mining and Mechanical Engineers." The iron manufacturer is so large a consumer of coke that any improvement in its manufacture closely affects his interest. The ovens almost universally used in this country for the preparation of coke are of that form known as the Beehive. Coppée's system is considered by the writer to present great advantages over these English ovens; but though it has been in operation on the Continent for at least a dozen years there have been but few of them erected in this country. The advantages of the Coppée ovens are—first, the retention in the form of coke of the largest possible proportion of the carbon of the coal; secondly, the utilisation of the heat of the evolved gases, by the use of flues so arranged as to impart an intense heat to the inside of the oven, and thus facilitate the expulsion of the gases; and thirdly, the application of the heat retained by the gases as they leave the

stack of the ovens to the production of steam. No water is introduced into the ovens, as is done in the old system, but the coke is watered after it has been withdrawn, and thus absorbs about 3 per cent of water.

MINERALOGY.

A mournful interest clings to a memoir in a recent number of "Poggendorff's Annalen," descriptive of Gustav Rose's researches on the action of heat on diamond and graphite. This memoir represents, indeed, some of the last work in the long and active life of the great mineralogist of Berlin—a life which extends over nearly seventy-five years, and was brought to a close on the 15th of last July. Although it is beyond our purpose to trace the history of Rose's scientific labours, we may yet point to the long list of papers in the Royal Society's Catalogue—a list numbering upwards of 120 memoirs—in proof of his extraordinary activity, his devotion to original research, and the success with which he cultivated mineralogical science, whether in its geometrical, its chemical, or its geological aspect.

Rose's last paper, which was communicated a few months ago to the Berlin Academy, is the outcome of some lengthened researches on the behaviour of diamonds at high temperatures. To some extent these observations confirm those of Schrötter and others who have worked in this direction. For example, Rose found that by placing crystals of diamond between carbon-points *in vacuo*, and subjecting them to the action of a Siemens's dynamo-electric apparatus, the diamond became red-hot, and eventually flew to pieces, and at the same time the surface acquired a black crust which had all the characters of graphite. Exposed to the temperature at which cast-iron melts, the diamond was found to undergo no change, but at the fusing-point of wrought-iron it became quite black and opaque, exhibiting a strong metallic lustre, and becoming, in fact, converted into a graphitic substance. But the most curious point in the behaviour of diamond is seen when the gem is heated in a muffle, with access of air. Under these circumstances the faces of the diamond exhibit regular triangular depressions, reminding one of the markings common on many of the South African diamonds. Some interesting examples of these symmetrically developed etchings are figured in the plates accompanying the memoir in "Poggendorff's Annalen."

As so much discussion has been rife with respect to the nature of the colouring-matter of the emerald,—one party referring it to an oxide of chromium and another to an organic source,—it is interesting to find that the subject has lately been taken up by Mr. C. Greville Williams, F.R.S., who has communicated his researches to the Royal Society. On exposing a South-American emerald to a bright reddish-yellow heat for three hours, in a platinum crucible, the green colour was *not* destroyed; hence the author was led to disconnect the question of colour from that of the presence of carbon. Indeed a colourless Irish beryl was found to contain rather more carbon than a richly-tinted emerald. The author believes that there is no room for doubting the correctness of Vauquelin's conclusion, that the green colour of the emerald is due to the presence of chromic oxide. Experiments on the fusion of beryl and emeralds showed that these gems lose density when fused, but this fact cannot be used in argument against the formation of such minerals at a low temperature; for it is quite possible that they were crystallised from a fused mass which was originally formed at a temperature sufficiently high to keep the constituents of the emerald in a state of fusion, and that the crystals developed during a slow process of cooling.

Von Kobell has lately described, under the name of *Kjerulfin*,—a name suggested in honour of the Norwegian mineralogist and geologist, Kjerulf,—a new mineral-species from Bamle, in Norway. The substance had been determined by Rode, of Porsgrund, to be a new phosphate of magnesia, and complete analyses by Von Kobell and Wittstein lead to the following formula:—$2(3MgO.P_2O_5)+CaF_2$. Kjerulfin is therefore very similar in composition to the rare mineral known as Wagnerite.

Having had his attention directed to Wagnerite, by the study of Kjerulfin, Von Kobell has undertaken a new analysis of the old phosphate. From this recent examination of Wagnerite its formula seems to be thus represented:— $2(3MgO.P_2O_5)+(\frac{2}{3}Na,\frac{1}{3}Ca)F_4$.

A new mineral, belonging to the Pinite group, has been described by Laspeyres, of Aix-la-Chapelle, under the name of *Hygrophyllite*. This name refers to the curious behaviour of the mineral when brought in contact with water or with steam. Placed in water, the mineral—which is ordinarily of a greenish tint—immediately becomes white, and exfoliates in very fine scales, which peel off, layer after layer, until the entire mineral is disintegrated, and a whitish-grey finely-divided plastic mass is obtained, which, under the microscope, is seen to be made up of very delicate scales. In many other liquids, such as alcohol, ether, hydrochloric and nitric acids, the mineral retains its coherence.

Some "Mineralogische Mittheilungen" have been communicated by Dr. Wibel, of Hamburg, to Leonhard and Geinitz's "Jahrbuch." One of these communications describes the occurrence of the rare mineral, lime-uranite (autunite), in the so-called Portuguese phosphorite. The presence of a mineral containing uranium in a substance whose origin is so often referred to organic agency is not without its interest. The author has analysed a sample of gold from Vancouver Island, with the following results:—Gold, 91·86; silver, 6·63; copper, 1·00; iron, 0·51. A third subject discussed by Wibel is the composition and formation of blue carbonate of copper (azurite). His analysis of a Siberian specimen gave—Cupric oxide, 69·66; carbonic anhydride, 24·26; water, 6·08.

It is proposed by Von Kobell to change the name of the mineral called *Montebrasite* by Des Cloiseaux into that of *Hebronite*, the mineral having long been known from Hebron, in Maine, U.S.

The rare mineral called *Jordanite*, from the Binnenthal, has been lately studied by Sipocz, whose analysis points to the formula $As_2Pb_4S_7$. The same chemist also publishes analyses of a Hungarian Bustamite and an East-Indian potash-mica.

So much discrepancy may be observed in the analyses of the oxychloride of copper called *Atacamite* that we are glad to see that the mineral has been lately re-examined by E. Ludwig. His analyses of some fine crystals from Wallaroo, in South Australia, give results indicated by the formula $Cu_2ClO_3H_3$. The author believes the hydrogen to be essential to the constitution of the molecule of Atacamite, and that it exists in the form of hydroxyl. He constructs the following constitutional formula, making the atom of copper tetratomic:—

$$\begin{array}{l}Cu(OH)Cl\\ \|\\ Cu(OH)_2.\end{array}$$

The author's analysis of Brochantite also points to copper as a tetrad.

Atacamite has likewise been lately studied by Tschermak, who has specially directed his attention to the alteration by which this mineral can be transformed into malachite, some fine pseudomorphs illustrating this change having been obtained from the Siberian copper-mines.

Artificial crystals of Atacamite have recently been obtained by Friedel, whose process renders it probable that some natural forms of this oxychloride may have been produced by the action of ferric chloride on cupric or cuprous oxides. These results were obtained during the author's examination of a new mineral, *Delafossite*, which is a combination of the oxide of iron and copper, corresponding to $Fe_2O_3.Cu_2O$, or perhaps to the simpler expression $FeO.CuO$.

In some mineralogical notes on the Far West, Prof. Silliman puts on record the existence of *Enargite* in Southern Utah; and of *bismuthine*, *wulfenite*, *orpiment*, and *realgar*, from the same territory. He also describes a new borate of lime from Nevada, under the name of *Priceite*.

A chemical examination of Staurolite has been undertaken by Rammelsberg. Two crystals were examined—one from Brittany, and the other from Pitkäranta, in Finland.

The occurrence of indium in zinc-blende, from several American localities, has been detected by Mr. H. B. Cornwall. From Roxbury, in Connecticut, a blende was obtained so rich in indium that it could be detected spectroscopically by examining the raw powdered blende, without treatment with acids according to Richter's method.

Several analyses of American minerals have been published by Prof. A. R. Leeds, in a recent number of "Silliman's Journal."

A new analysis of *Dewalquite*, from Salm-Château, in Belgium, has been recorded by M. Pisani. This is the same mineral which has been described by German writers as *Ardennite*. The new analysis shows 3·12 per cent of vanadic acid.

ENGINEERING—CIVIL AND MECHANICAL.

Guns and Armour.—It is impossible to anticipate when or where the contest between guns and armour will cease. No sooner has an armour-clad vessel or fort been constructed, with a view to defence against the heaviest known guns, than the War Department at once begins to devise a gun whose shot shall pierce the thickest armour in the world. Thus there is continually going on an incessant competition for supremacy between the Admiralty and the War Office. The biggest gun of which we have hitherto heard is the "Woolwich Infant," of 35 tons weight, which fires a 700-lb. shot with a charge of 110 lbs. of powder; and the shot from this monster piece of ordnance could perforate the turret of the *Devastation* at any distance up to 500 yards. The armour plating of the *Devastation* is 14 inches in thickness, but an additional 2 inches in thickness would, it is said, render the vessel shot-proof against the biggest gun in the world. The War Department has hitherto cautiously advanced from guns of 12 tons weight, to 18, 25, and ultimately to 35 tons in weight; but they are now said to be contemplating the construction of one of 60 tons, the powder charge for which will weigh 200 lbs., and it will throw a shot over half a ton in weight, which will be able to perforate a 20-inch turret.

Harbours (Holyhead).—The inauguration of the harbour of refuge at Holyhead, on the 19th of August last, by the Prince of Wales, marks the completion of one of the finest works of this class yet completed. Between the years 1835 and 1847 the attention of Government was directed to the importance of providing improved harbour accommodation on the coast of North Wales, in the interest of the packet-service between England and Ireland. For this purpose Holyhead was selected as the most suitable site; and of the several schemes proposed for that place to accomplish the required end, the plan suggested by the late Mr. James Meadows Rendel was ultimately accepted, who, in August, 1845, was requested by the Lords of the Treasury to furnish detailed plans and estimates for the new harbour, and who reported thereon on the 5th of December of that year. Mr. Rendel's plan consisted of a north breakwater 5360 feet in length from the coast line, and an eastern breakwater about 2000 feet in length,—the two enclosing between them an area of 267 acres of available water space,—and of a packet pier 1500 feet long, situated within the enclosed area. The east breakwater was subsequently abandoned, as was also the proposed packet-pier, it having been subsequently determined to carry on the packet-service in the old harbour, where jetties and works have been constructed for the purpose. The north breakwater alone has been retained, and the new harbour has become principally a harbour of refuge. As the works advanced, however, it was found that, notwithstanding the abandonment of the new harbour as a packet station, which increased its capacity as a harbour of refuge, it was likely to prove too small even for refuge purposes. The Lords of the Admiralty therefore decided to lengthen the north breakwater by 2000 feet, and subsequently by another

500 feet, making its total length 7860 feet from the shore. These extensions considerably more than doubled the capacity of the harbour for refuge purposes, for they sheltered a roadstead of 400 acres of deep water, in addition to the 267 acres of water space. The breakwater consists of a sub-structure, or rubble mound, of stone, upon which is erected a substantial stone superstructure, the end of the breakwater being terminated by a head, on which is erected a lighthouse. The rubble mound is of great size, the average depth of water at low-water spring tides being 40 feet, and the greatest depth 55 feet, the rise of tide being 18 feet. The inclination given to the foreshore, or the slope from low water to the superstructure, is nowhere steeper than 7 to 1, and this inclination continues to about 10 feet below low-water mark, when the mound assumes a slope of 2 to 1, to about 25 feet below low-water mark, and somewhat flatter than 1 to 1 from that point to the bottom. On the harbour side the slope of the mound is about 1 to 1. At the level of low water the mound is nowhere less in width than 250 feet, and in 50 feet depth of water it is 400 feet wide at the base. It contains altogether about seven millions of tons of stone. The superstructure consists of a solid central wall of massive masonry, built principally of stone from the Holyhead mountain quarries. The foundations of this wall are laid at the level of low water, and it is carried up to a height of 38 feet 9 inches above low water, upon which is a handsome promenade, surmounted on the sea side by a massive parapet. At a lower level, or at 27 feet above low water, there is on the harbour side of the central wall a lower terrace or quay, 40 feet wide, formed by an inner wall built at a distance from the central wall, the intermediate space being filled in with suitable material. The head at the end of the breakwater is a massive structure, 150 feet long and 50 feet wide. The first contract for these works was made on the 24th of December, 1847. The late Mr. Rendel was Engineer-in-Chief from the commencement of the work until his death, at the end of 1856, when Mr. John Hawkshaw was appointed to that post, and the works have since been carried on under his superintendence. The cost of the whole of the works has been £1,479,538, which includes not only the outlay on the north breakwater, but also the provision of the accommodation for the Irish Postal Service in the old harbour, the construction of a beaching ground, and other miscellaneous works.

Kurrachee Harbour.—These important harbour works in India having recently been brought to a completion, a brief notice of them will be interesting. Kurrachee is situated near the north-western extremity of Sindh, and is the only seaport of that province available for vessels drawing more than 10 feet of water. Its position is one of very great importance, whether regarded from a commercial, political, or military point of view. In 1848 a lighthouse was erected on Manora Point, on the western entrance to the harbour; and, in 1855, in consequence of the increasing importance of the place, two dredging vessels were constructed for the improvement of the harbour, and a light-draught steamer was provided for the purpose of towing vessels in and out. In 1856 a committee was formed on the spot for the purpose of considering the best means of effectually improving the harbour, and their report was referred for the opinion of the late Mr. James Walker, C.E., who, in October, 1858, submitted his report, in which he recommended the construction of a breakwater from Manora Point, on the west side of the entrance; of a groyne from Keamari, at the eastern side; the closing of Chinna Creek, so as to force the ebb and flow tides to pass up and down the entrance channel, and the construction of new docks and basins, and of a graving dock. In 1860 orders were issued for the commencement of the works, which, with certain interruptions, have been carried on up to the present time,—first under the direction of the late Mr. Walker, and at his death by Mr. W. Parkes. Keamari Groyne is 7400 feet long, constructed of stone upon the Keamari sand-spit, having its top 2 feet 6 inches above monsoon high-water. This work was commenced in December, 1861, and completed in April, 1863. The extension of the groyne for 1500 feet (known as the East Pier) was commenced in May, 1864, and completed in October, 1865. Other works within the channel were also carried out which had the effect of considerably improving the

harbour, but it was not until the year 1868 that the construction of the Manora Breakwater was sanctioned. This breakwater is 1500 feet in length, running out into the sea in a direct line from Manora Point. It is constructed of huge blocks of concrete resting upon a rubble base. These blocks are all formed of a uniform size and shape, and are held in position merely by their own weight, no cement of any kind being employed to join them to one another. The last block was set, and the Kurrachee Harbour Works thus far completed, on the 22nd of February last. By the aid of dredging over the bar, which runs across the mouth of the harbour, the result of these works is an increase of the depth of channel to 20 feet, and a considerably increased water-space within the harbour available for vessels to moor in.

International Communication.—M. Dupuy de Lôme has recently contributed to the Academy of Sciences a paper upon the scheme he has elaborated, in connection with Mr. J. Scott Russell, for improving the means of communication between England and France. According to the author of the paper, it is by the improvements in ships and ports that the desired end can be achieved rapidly, and without financial assistance from the State. The proposed solution to the problem involves the employment of very large vessels, suitable for carrying passenger and goods trains on board, as long since proposed by Mr. Fowler. The dimensions of these ships would be—Length, 442 feet; breadth, 35·75 feet; load draught, 11·48 feet; displacement, 2700 tons. They would be driven by side wheels 32·8 feet in diameter, actuated by engines of about 3600 horse-power. Each vessel would be able to carry a train, either for passengers or goods, 380 feet in length, and weighing 300 tons for the goods or 180 tons for the passenger train. The train would be run upon the after part of the ship, upon rails laid on the lower deck, and at a level of about 6 feet 6 inches above the sea. It would thus be covered by the upper deck, and be securely sheltered from the action of the sea. On either side of the line of rails suitable saloons and state-rooms would be provided for the accommodation of passengers. According to M. Dupuy de Lôme the steadiness of the boats would be greater than it is possible to obtain with ordinary ocean steamers: the mean intervals of the channel waves being from 7 to 8 seconds, the vessel should have a period of oscillation of from 12 to 13 seconds, so that one wave would counteract the rolling produced by the previous one. On the English coast Dover Harbour has been selected as a terminus; but as on the French side there exists no harbour like that of Dover, suitable for the entry of large vessels at all stages of the tide, it is proposed to create a port appropriate to the service of the train ferries, and the site selected by M. Dupuy de Lôme and Mr. Scott Russell is in the locality of Calais, the port being so laid out as not to produce any silting up around the entrances.

Dover Harbour.—The importance of improving Dover Harbour has at last become so urgent, both in consequence of requirements for improved accommodation for the Continental service, and of the necessities of the port for naval and military purposes, that the Government recently requested Mr. Hawkshaw to place himself in communication with Colonel Sir Andrew Clarke, with the view of considering whether a plan could be prepared which would combine the naval and military requirements of Dover with the objects desired to be effected by the Dover Harbour Board, and what the works would cost. The result has been a joint report from those gentlemen which, for the packet service, proposes the construction of a steam-packet pier about 1250 feet long by 125 feet wide, starting from a point close to the Admiralty Pier, and running in a south-easterly direction. Besides this a breakwater is proposed, which commences about 400 feet to the east of the Castle Jetty, and is continued seawards in a slightly south-westerly direction for about 3800 feet. At that point it turns and runs west for about 2200 feet, when it ends. Then comes an opening of 600 feet for vessels to enter, at the other side of which will be the end of the proposed extension of the present Admiralty pier, which is a length of 500 feet, and meets the end of the work now in progress at the present head of the pier. An opening of 900 feet is

left in the eastern end of the breakwater. The estimate for the breakwater alone is, according to this design, £850,000, and the maximum time of construction eight years. Colonel Clarke has, however, proposed an alternative design, in which the railway company's water station is retained in a modified form. The proposed extension of the Admiralty Pier eastwards for 500 feet is retained, and then comes an opening of 600 feet. The breakwater then commences, and turns eastwards for about 2000 feet, when it turns by a curve, and takes a northerly direction to the shore, which it joins about 150 feet eastward of the Castle Jetty. The eastern entrance is omitted, as owing to the currents at that point it would in all probability silt up. The area enclosed is about 350 acres to low-water line. In the space left between the Castle Jetty and the eastern wall of the breakwater, Colonel Clarke proposes to build a small dockyard. From the Castle Jetty, along the whole coast line to the entrance of the present harbour, Colonel Clarke further proposes in the future to have a commercial quay and boulevard with trees and promenades. The whole matter is in the hands of the Board of Trade, and a sum of £10,000 was voted last session for carrying out the work according to Colonel Clarke's alternative design. The breakwater will be formed of concrete blocks, with the intervening space filled in with fluid concrete to a level of about 16 feet below low water. From that point and up to 3 feet above low water, concrete blocks alone will be used. From the last-named level to the top of the work, which is 6 feet above high water, the structure will be carried out in concrete, which will be put in between tides. The modification proposed by Colonel Clarke will admit of the completion of the work in five years from the time of their actual commencement.

Bow and Stern Screw Ships.—A paper on this subject was read on the 16th of June last, before the Royal United Service Institution, by Mr. Robert Griffiths, whose name is so well known in connection with the subject of screw propulsion. In order to avoid some of the difficulties and dangers that now attend screw ships, and also to improve their speed, it occurred to Mr. Griffiths that if in addition to the screw at the stern another propeller were applied in the bow of a steamship, both screws being placed in tunnels formed in the side of the ship so as to be protected from coming in contact with such objects as a ship's anchor or cable, it would be the means of avoiding a great many of the difficulties and dangers now attendant upon screw ships. From experiment it was ascertained that the best arrangement was that wherein the water from the bow screw was delivered underneath the ship, and water for the stern screw was taken also from underneath, so that both ends of the ship were made the same below the water-line. In this case the bow screw itself gave a better result in consequence of the water discharged from the screw meeting with a greater resistance, giving the same effect as is now produced by lowering the screw, and thereby obtaining a deeper immersion of the blades. Another great advantage may be obtained by this method of placing the screw, and that is, that the screw may be made to discharge any bilge water or any great leakage that may happen to take place in the vessel. As a result of trials made with a small vessel thus fitted with bow and stern screws, Mr. Griffiths asserts that besides numerous other advantages from this method of disposing the screws, 20 per cent less power is required by the screw when working in the tunnel to obtain the same speed than when working in the ordinary way.

Rock Drills.—A paper on the Diamond Rock Drill was recently read before the Iron and Steel Institute at Liège by Major Beaumont. The diamond drill is in principle quite distinct from any other system of holing rock, and works by rotation without striking a blow. Its action is rather that of abrading than cutting, and the effect is produced by the sheer difference in hardness between the diamond and the rock it is operating upon. The wonderfully resisting power of the diamond enables machinery of the simplest and most ordinary character to be used, and thus avoids those special difficulties that the mechanic must face when he is driven to utilise a large power in the production of percussive action; moreover, machinery can be applied in places where a reciprocating motion, if admissible at all, would present

peculiar difficulties. The diamonds that are used are not valuable gems, but carbonate, a substance that till lately had no commercial value, and was first introduced for the purpose of cutting other diamonds. It differs from the brilliant diamond in being very imperfectly crystallised, which also gives to carbonate its value for drilling purposes, as it has next to no cleavage, and consequently does not split up or break in the way that a diamond or piece of boart would do. The application of the diamond to rock-drilling is worked out as follows:—The stones are set in an annular ring made of steel; they are fastened in by making holes as nearly as possible the size of the stones to be set, and then burying them, leaving projecting only the amount necessary to allow the water and *débris* of the cutting to pass; the metal is then drawn round the stone so as to close it in on every side, and give as large a bearing surface as possible to resist the tendency of the stone to be forced out. The crown so set is attached to the end of a steel tube and kept rotating against the rock at some 250 revolutions per minute. Water is supplied through the hollow of the bar, whence it passes under the cutting face of the crown to the surface of the hole between the side of the latter and the outside of the boring tubes; the diamonds are thereby kept cool, and the *débris* from the cutting is washed away. The crown has to be kept pressed forward with a force depending on the nature of the rock to be cut, varying from 400 lbs. to 800 lbs., when the cutting is done at speeds varying from 2 inches to 4 inches per minute. Granite and the hardest limestone are readily cut at 2 to 3 inches per minute; sandstone at 4 inches; and quartz at 1 inch per minute. The cutters travelling in an annular ring, it follows that a core is produced, an arrangement which, while it ensures a minimum of work being done to make a given sized hole, affords evidence of the strata passed through, a fact which is invaluable for certain applications.

GEOLOGY.

Palæontology.—Mr. Ray Lankester has described a new form of heterostracous fish-shield, which is intermediate in form between *Scaphaspis* and *Pteraspis.* The specimen, which he names *Holaspis sericeus*, is figured in the "Geological Magazine," and was discovered in the grey cornstone, of Old Red Sandstone age, near Abergavenny.

Professor Owen has communicated to the Geological Society the description of a fossil dentigerous bird, which he names *Odontopteryx toliapicus.* From a consideration of all the characters furnished by the remains, which were obtained from the London clay of Sheppey, he concluded that it was a warm-blooded feathered biped with wings; that it was web-footed and a fish eater, and that in the catching of its prey it was assisted by the pterosauroid armature of its jaws. He indicated the characters separating *Odontopteryx* from the cretaceous fossil skull lately described by Professor O. C. Marsh,* and which he affirms to have small similar teeth implanted in distinct sockets.

Professor Duncan, continuing his researches on the fossil corals of the West Indies, has now described those from the Eocene formation. He remarks that the affinities and identities of the fossil forms with those of contemporaneous reefs in Asia and Europe, and the limitation of the species of the existing Caribbean coral fauna, point out the correctness of the views put forth by S. P. Woodward, Carrick Moore, and himself concerning the upheaval of the Isthmus of Panama after the termination of the Miocene period.

In an address to the Natural History Society of Montreal, Dr. Dawson has discussed the geological distribution of the oldest known fossil, *Eozöon Canadense* and allied forms. He mentions its occurrence in rocks of Huronian age in Ontario and Bavaria; in the Middle and Upper Cambrian there are few limestones likely to contain such a fossil, but in Labrador species of *Archæocyathus* are found, one of which he has ascertained to be a calcareous chambered organism of the nature of a foraminifer, though there is little doubt that others are, as Mr. Billings has shown, allied to sponges. In the limestones of the Trenton group (Lower Silurian) animals of the Eozöon type occur abundantly. The concentrically laminated fossils which sometimes

* *Vide* Quart. Journ. Science, No. xxxviii., April, 1873, p. 272.

form large masses in these limestones, and which are known as *Stromatopora*, are mostly of this nature, although fossils of the nature of corals have been included with them. In the Upper Silurian (or Silurian proper) are similar if not identical forms known as *Cœnostroma*, with a skeleton consisting of a series of calcareous layers connected with each other by pillars or wall-like processes; while in the Devonian masses of limestone, sometimes 12 feet thick are made up of these organisms, which have clearly foraminiferal affinities, and are intermediate between the Eozöon of the Laurentian and the *Parkeria* and *Loftusia* of the greensand and eocene.

Prof. Bianconi has published further information on the bones of *Æpyornis*, corroborative of his views of its being an immense vulturine bird, the "Roc" of Marco Polo.

M. Schmidt, in a note published in the "Geological Magazine," states his opinion that the shields of *Pteraspis* and *Scaphaspis* belong to one and the same animal, *Scaphaspis* representing the ventral shield of *Pteraspis*.

Mr. Davidson has described some Brachiopoda collected by Mr. Judd from the Jurassic deposits of the east coast of Scotland. Three of them were obtained from the equivalent of the Kimmeridge Clay, which was the more remarkable as the Brachiopoda of that formation are comparatively few.

Dr. Gümbel has described to the Bavarian Academy of Sciences the so-called "Nullipores," which he resolves into two kinds:—1. True calciferous Algæ (*Lithothamnium*, and an allied form, *Lithiotis*); and 2. Foraminifera (*Dactyloporideæ*).

M. Barrande, who is so well known for his researches on the Silurian system of Bohemia, has recently published a supplement to Vol. I. of his great work, on the different Crustacea and Fishes of these old rocks. Of the former he describes ninety-four new species of Trilobites, of the latter indications of four genera have been discovered, namely *Asterolepis*, *Coccosteus*, *Ctenacanthus*, and *Gompholepis*. His observations do not agree with the theory of evolution. He passes in review the different parts of the Trilobites, the succession of their species and genera in time, and institutes a comparison between the fishes, Trilobites, and Cephalopoda, and their relations to the primordial fauna generally. Everywhere he finds that the appearance of new forms is sudden and unaccountable, and that there is no indication of a regular progression by variation.

The base of the Palæozoic series in America until lately was formed by the Potsdam sandstone, although lower horizons of life had been determined by Barrande in Bohemia, and Salter and Hicks in Wales. The researches of Mr. Murray in Newfoundland, together with the study of the fossils by Mr. Billings, have revealed a lower Potsdam, while Messrs. Hartt and Matthew, by their explorations of the rich primordial fauna of St. John, have led to the establishment of an "Acadian Group" on the horizon of the lower slate group of Jukes in Newfoundland, of the gold-bearing rocks of Nova Scotia, and of the slates of Braintree in Massachusetts.

Stratigraphical Geology.—One of the most important papers in British geology which has been published in recent years is that "On the Secondary Rocks of Scotland," by Mr. J. W. Judd, the first part of which has been published in the "Quarterly Journal of the Geological Society." The Mesozoic periods are in Scotland represented only by a number of isolated patches of strata, situated in the Highlands and Western Isles; which have been preserved from the destructive effects of denudation, either through having been let down by great faults among the palæozoic rocks, or through being sealed up under vast masses of tertiary lavas. These have been unravelled by Mr. Judd, who has depicted their superficial extent upon a coloured geological map which accompanies his paper. The cretaceous rocks, exhibiting very interesting characters and yielding a beautiful series of fossils, were discovered by the author on the mainland, and in several of the islands of the west of Scotland. The Jurassic rocks, which were first described by Murchison, are now shown to present a remarkable contrast with their equivalents in England,

in being constituted throughout their whole thickness by alternations of marine and estuarine series of beds; in which respect they precisely resemble the equivalent strata of Sweden. The Triassic rocks have now been discovered in Sutherland, where their conformable relations to overlying beds, containing a fine Liassic fauna, entirely confirms the conclusions concerning their age, derived from Professor Huxley's studies of the remarkable reptiles yielded by them in Elgin. Two new species of Brachiopoda, discovered by Mr. Judd in the Upper Oolite of Garty, in Sutherland, are named by Mr. Davidson respectively, *Rhynchonella Sutherlandi*, after the Duke of Sutherland; and *Terebratula Joassi*, after the Rev. J. M. Joass.

Physical Geology.—The Rev. J. M. Mello, in a sketch of the geology of Derbyshire, touches upon the solvent power of water on the mountain limestone as explaining the origin of its characteristic scenery. He considers that the dales were in many instances originally caverns, which have been through countless ages eaten away by the streams till at length the roofs have fallen in, and in their turn have been for the most part carried away by the same powerful agent.

The Rev. O. Fisher, in treating of the formation of mountains, has attributed the elevating force, which has raised mountain ranges, to the contraction of the heated interior of the earth, and subsequent wrinkling of the crust so as to accomodate itself to the diminished nucleus. In a recent paper, communicated to the "Geological Magazine," he proves that if we suppose a stratum 500 miles thick, buried under 25 miles of crust, to have contracted since the crust became rigid on the whole, as much as a slag would do in passing from a fused to a devitrified state, this would give a mountain-range of something under half a mile high on every hundred miles of surface. If only a part of the area were disturbed the mountains would be higher.

Mr. W. T. Blanford has drawn attention to the superficial deposits of Persia. He described especially the desert plains of the interior of the country, the paucity and scantiness of the streams, most of which terminate in salt swamps and lakes, and the occurrence of vast slopes of gravel on the margins of the desert plains, covering up the junction of the latter with the surrounding mountains. The desert plains he regarded as in general the beds of ancient lakes.

Glacial Geology.—The Duke of Argyll, in his Presidential Address to the Geological Society, while discussing the general opinion in regard to the glaciation of the British Isles, remarked "that the history of geology, like the history of other sciences, is the history of the prevalence of particular theories at particular times—not generally to be wholly abandoned, but almost always to be greatly modified." He had "a strong impression that the glacial theory is now at about its maximum, and that, when all our valley-systems are described as being nothing but magnified striæ, we are pretty near the summit-level of this particular excursion of the scientific imagination."

Dr. Dana's observations upon the Glacial and Champlain Eras in New England, go to show that the former was an era eminently of transportation by ice, the latter one of deposition. He regards the Glacial period as of great duration, and expresses the opinion that 1 foot a week was the average rate of the movement of the ice, so that 10,000 years would be required to carry a boulder 100 miles. In the northern part of New England, he estimates the ice to have had a thickness of from 5000 to 6500 feet, and in the southern part an average of 2700 feet. The pressure must have been immense—6000 feet corresponding to at least 300,000 pounds to the square foot; the glacier as it moved must have had tremendous power in abrading, and made boulders and gravel in immense quantities.

Mr. J. C. Ward has described the glaciation of the Northern part of the Lake district. He maintains that there is no evidence that a great ice-cap from the north ever swept over this district. The ice scratches trending along the principal valleys, but sometimes crossing watersheds, indicate a great confluent glacier-sheet, at one time almost covering a great part of the district,

the movement of which was determined by the principal watershed of the Lake district.

Mr. J. F. Campbell has described the Glacial phenomena of the Hebrides. Various ice-marks were noticed, which all seemed to come from the north and west, also numerous perched blocks. On the whole, the author was inclined to think that the last Glacial period was marine, and that heavy ice came in from the ocean, the local conditions being like those of Labrador. He regarded most of the Lake-basins of the Hebrides as formed by ice-action, and considered that the ice by which those islands were glaciated came from Greenland. Mr. James Geikie has also described the Glacial phenomena of Long Island, or Outer Hebrides. The lakes of the mountain district he regarded as all produced by glacial erosion.

The Duke of Argyll has discussed the formation of six Lake-basins in Argyllshire, five of which he considers could not have been due to glacial action.

The glaciation of the British Isles has received of late a good deal of attention, and the presence of sheets or fields of ice covering vast areas has been invoked in order to account for the phenomena observable in different parts of the Kingdom. Mr. R. H. Tiddeman, describing the glacial phenomena of North Lancashire, and the adjacent parts of Yorkshire and Westmoreland, as bearing testimony to a widespread and almost universal glaciation in the district, brings forward evidence to show that they were produced by an ice-sheet. While, however, the drainage of the district is to the south-west, the general movement of the ice over it appears to have been to the south or south-south-east, across deep valleys, and over hills of considerable elevation. This he explains by the scratches on the rocks, the direction and method of transport of the Till, its materials, and their arrangement along lines coinciding with the scratches, as well as by the superficial disturbances of the rocks. To account for the direction of the ice-sheet, which under ordinary circumstances would be working down from the watershed to the sea in the direction of the main valleys, Mr. Tiddeman considers that there must have been a great barrier, along what is now the seaside plain, to dam up the mouths of these valleys to a great height, and prevent their discharge of ice to the south-west. Evidence of such a barrier exists in the traces of a great stream of ice coming from the Lake district and bearing with it rock specimens of that country. This barrier he concludes was but the line of junction of the ice of the Pennine chain with that from the Lake district, and to the eye they must have presented only the appearance of one great sea of ice. These observations, which Mr. Tiddeman communicated to the Geological Society of London, are illustrated by a coloured map upon which the physical features of the country are depicted and the direction of the ice-scratches shown.

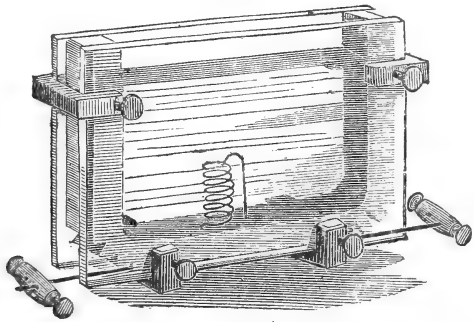
PHYSICS.

Microscopy.—The most powerful spectroscope yet constructed has been presented to the University of Oxford by Mr. J. P. Gassiot. The great dispersive power of the instrument is obtained by a battery of six compound prisms 3 inches high by 2 inches wide. The light, after passing through the upper half of these prisms, is reflected back through the lower half, the light in its course through the prisms having to pass through more than 4 feet of glass before it reaches the eye of the observer. The telescopes are of 18 inches focal length, and the object-glasses 1¾ inches in diameter. The prisms are provided with the automatic arrangement for keeping them at the minimum angle of deviation for any ray under examination. It is intended that all the measuring of the spectra should be done by means of a micrometer eye-piece placed in the telescope; but for the purpose of readily finding any line in the spectrum, the prisms are provided with a vernier which moves round a circular arc; the divisions are on an alloy of palladium with silver. There is a contrivance for setting the train of prisms in motion, the milled head which moves the prisms being close to the eye-piece of the telescope, and thus completely under the command of the observer. The weight of the instrument is rather more than 140 lbs.

Mr. J. W. Stephenson, F.R.A.S., and Mr. Charles Stewart, F.R.M.S., make use of the appearances presented by objects immersed in media of different refractive power to determine some points in their structure.* This especially applies to colourless transparent organisms such as the skeletons of diatoms and siliceous and calcareous spicules of sponges. The siliceous deposits, both of plants and animals, are of less refractive index than Canada balsam ; consequently, when mounted in that medium they appear, if convex, to act as concave lenses do in air, and *vice versa.* If diatoms are examined in air, *i.e.*, dry, they are in some instances too opaque for transmitted light, but on immersing them in water, of which the mean index is 1·366, they become more translucent ; with media of higher refractive power, the translucency increases until the mean index of strong sulphuric acid (1·434) is attained, in which they become practically invisible. As every object which is transparent and colourless becomes absolutely invisible when immersed in a colourless medium identical in refractive power with itself, we know approximately that the refractive index of diatomaceous silex is 1·434 (much below that of quartz), and this is accordingly for diatoms our neutral point. By progressively increasing the refractive power of the mounting medium the diatoms again become more and more visible, until, as we all know, when mounted in Canada balsam (1·540) the coarser species are sufficiently defined for all ordinary purposes ; but if we require a still greater departure from the neutral point, or invisible condition, we must select some other substance of still higher refractive power. This we find in bisulphide of carbon, the index of which is 1·678, and by dissolving phosphorus in the bisulphide we may obtain any power between 1·678 and 2·254 ; but when large thick diatoms, such as *Heliopelta*, are mounted in a strong solution of phosphorus, they again become nearly, if not quite, as opaque as they were in air. From the above it is evident that, on examination of a diatom or other object in air and in bisulphide of carbon, they are seen under conditions in which the respective optical effects arising from their form are reversed. The results of the examination of some diatoms are given in the paper, with figures showing the various effects produced by the use of varied mounting media. It is also suggested that some animal tissues, in which the staining process has failed to reveal differences of structure, may be profitably examined in media of such high refractive index as bisulphide of carbon or oil of cassia.

Lecture Illustrations of Solar Phenomena.—In a recent lecture on "Sunlight and its Source," President Morton, of the Stevens Institute of Technology, employed several new illustrations of his own device, which will interest some of our readers. In the first instance, to illustrate the motion of sun

FIG. 4.

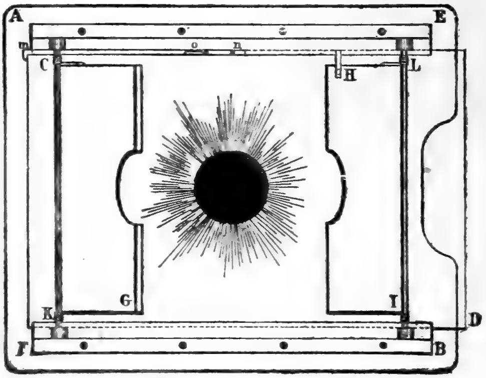


spots across the solar disc, their foreshortening when near the limb, &c., an apparatus was employed consisting of a glass cylinder, on which a sun spot

* Monthly Microscopical Journal, vol. x., p. 1.

was painted, supported so as to rotate on its axis and admit of various inclinations. This was placed as an "object" in a large oxyhydrogen lantern, in front of a circular orifice representing the solar disc. The effect upon the screen was remarkably good. To imitate the formation of solar protuberances, a glass tank, provided with a coil of platinum wire, was likewise introduced in the lantern and filled with water, at the bottom of which was a little solution of cochineal. The coil being heated by the current from a single "flask" battery, a stream of the crimson cochineal solution was thrown up, and assumed from time to time various forms, some of which bore a striking resemblance to the figures of solar prominences which have been figured by Young, Lockyer, Respighi, and others. Again, to illustrate the various phenomena of a solar eclipse, an apparatus was employed whose construction and operation can be explained by the aid of the accompanying figure. To the further

Fig. 5.



side of the frame, A B, is attached a plate of glass on which is painted or photographed a picture of the sun's disc, with the "flames" and corona. These are, of course, bright on a dark ground. Next, in front of this, slides a plate of clear glass, C D, with a brass disc at its centre, of such size as to correctly represent the moon's apparent diameter as compared with that of the sun. The edges of this disc are slightly serrated, to represent the mountainous profile of the moon as shown in some of the eclipse pictures. In front of this plate are arranged two doors opening on hinges, C K, and L I, and having spiral springs also at these points, which tend to throw them open. The door, H I, a little overlaps the other, and thus a bolt, *m o n*, engaging a projection at H, secures both doors when they are shut. A circular orifice, half in each door, corresponds with the solar disc on the rear glass. The doors then being shut, and the plate C D drawn to the right, we see only the bright solar disc with such sun spots and faculæ as have been represented on it. Then the plate C D being slowly pushed towards the left, we see the moon's disc encroaching on the sun, and all the phenomena of the partial phases, ending with "Baily's beads," which are of course due to the serrations of the disc. An instant after, and as the disc entirely shuts off the sun, the glass plate, C D, by pressing against the lug, *m*, of the bolt, shoots it, and allows the doors to fly open, displaying the prominences and corona surrounding the dark lunar disc. The figure represents the doors in the act of flying open, and is correct in detail, except that the corona should appear bright on a dark ground. Yet again, in illustration of the vivid brightness of the crimson protuberances or hydrogen flames, as seen during a total eclipse, the following device was employed:—A large coloured drawing of the moon, surrounded by the solar corona, was stretched on an appropriate frame, and the places of the prominences cut out. Behind these were attached a number

of hydrogen spectrum tubes, and the whole enclosed from behind with a board covered with white paper. The reflection from the white paper fills the whole opening representing the prominence with crimson light, so that the tubes are not noticed. Fig. 6 shows the general appearance, and Fig. 7 the arrangement

FIG. 6.

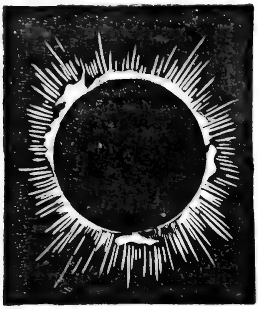
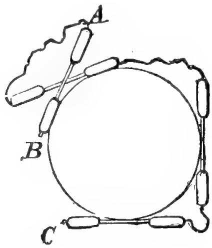


FIG. 7.



of the tubes. President Morton also stated that he was preparing some spectrum tubes with meteoric hydrogen, so as to have what might be considered as a real solar-prominence specimen.

ELECTRICITY.—The improvements in magneto-electric machines follow each other in rapid succession. Hardly is M. Gramme's machine announced ere Mr. Wild, who has long been known in electrical circles as one devoting great attention to, and bringing forward important inventions in, magneto-electricity, introduces a machine with multiple armatures, producing a greater number of currents for one revolution of the axis than has hitherto been obtained. Although this machine has for many practical purposes been superseded by the continuous current generator of M. Gramme, its importance in the production of the electric light must not be overlooked. It consists of a circular framing of cast-iron firmly fixed by stay-rods. A heavy disc of cast-iron is mounted on a driving-shaft running in bearings fitted to each side of the framing; one of these bearings is carefully insulated from the framing by ebonite, and also from the shaft by a cylinder of the same substance. Through the side of the disc, and parallel with its axis, sixteen holes are bored for the reception of the same number of cores or armatures. Around each inside face of the circular framing, and concentric with the driving-shaft, sixteen cylindrical electro-magnets are fixed; the two circles of magnets consequently have their poles opposite each other, with the disc and its circle of iron cores revolving between them. The ends of the cores are terminated with iron plates of a circular form, which answer the double purpose of retaining the helices surrounding the cones in their places, and overlapping for a short distance the spaces between the poles of the electro-magnets. The closing of the *magnetic* circuits of the electro-magnets and armatures for a short distance, like the closing of the *electric* circuits for a brief interval at the point of no current, has a marked influence on the power of an electro-magnetic induction machine, both contrivances conspiring simultaneously to maintain the magnetic intensity of the electro-magnets during the rise and fall of the magneto-electric waves transmitted through the helices. With the Gramme machine much progress has been made in detail. One of the improvements, by Mr. Robert Sabine, C.E., tends to add mechanical, and somewhat of electrical, strength to the apparatus. Instead of the cumbrous magnets employed by the inventor, Mr. Sabine proposes to substitute magnets placed parallel to the coil (see Figs. 3 and 5 of our article on "Magnetic Illumination" in the July number of this Journal) and united at their similar poles by transverse bars of soft-iron. The magnetic polarity of these bars follows precisely the Faradian lines of force, and the true pole appears to be situated in, or rather

shifted to, the point where the bar would most influence the coil. The advantages gained in economy of construction (by the use of smaller magnets) and in compactness of arrangement are the chief points in the improvement. With the Gramme machine a striking experiment, first made by MM. Gaston-Planté and A. N. Breguet, has been recorded by them in "Les Mondes." They charge a Planté secondary couple (coiled sheets of lead in dilute sulphuric acid), and, in place of discharging the couple, they allow it to remain in communication with the machine; when, if the machine be suddenly and completely stopped by the hand, the coil, on the removal of the hand's resistance, will re-continue to revolve, not in an opposite direction, but in the same direction as when charging the secondary couple, for a period of two or three minutes. Nothing would appear more paradoxical than that the machine should continue to turn in the *same* direction. But the explanation is simple, and is given by the experimenters in the following words:—"If we consider the direction of the current furnished by the machine that of the current given back by the secondary couple, which is inverse to the preceding, and if we take into account the action which results, we shall find that, according to the laws of induction and electro-dynamics, the movement of rotation should be in the direction indicated by experiment. And if we observe that the secondary couple has an intensity temporarily superior to that furnished in a given time by the machine, we shall easily be able to perceive that the discharge from the secondary couple overcomes that of the machine."

The next step in electrical science most probably will be in improved application of the electric light. Recently, at St. Petersburg, experiments have been made by Messrs. S. A. Kosloff and Co. with the invention of M. Ladiguin. This invention consists in the use of one piece of charcoal or other bad conductor, which, being attached to a wire connected with a magneto-electric machine, is placed in a glass tube from which the air is exhausted, and replaced by a gas which will not at a high temperature combine chemically with the charcoal. This tube is then hermetically sealed, and the machine being set in motion, the charcoal becomes gradually and evenly heated, and emits a soft, steady, and *continuous* light. Taking into consideration the fact that one machine worked by a small three-horse power engine is capable of lighting many hundred lanterns, it is evident that a great step has been gained to the end of lighting our streets by means of electricity.

Practically, perhaps, the greatest good has been achieved by the application of the duplex system of working on our telegraphic lines and cables. By this system the capacity of our lines for work are doubled, and consequently the capital invested should repay doubly what it would on the single transmission system. The improvements in this country come chiefly from Mr. W. H. Preece, C.E., of the Post Office telegraphs. The Americans have long employed a system similar in effect, invented by Mr. J. B. Stearns. The system apparently most in favour is that founded on the use of the Wheatstone bridge; and, indeed, the system may be described briefly as the arrangement at each station of such a bridge system as will have its galvanometer needle deflected by a current other than that of its own battery. This is effected by substituting the instrument of the sending station for the galvanometer of the bridge, putting equal or proportional resistances into two branches, the line wire into the third branch, and a resistance compensating that of the line wire into the fourth. An out-going current thus possesses opposite and equal potentials at the terminals of the instrument, but an in-coming current finds only a divided circuit, in one branch of which is the instrument.

Amongst the electrical apparatus meeting the wants of the laboratory and lecture-table especially, may be classed the new ozone-generator invented by Mr. Tisley. The construction of this instrument may be described as follows:—On each end of a piece of glass tube of uniform bore is placed a brass cap, bored with two holes, and coated internally with shellac. In the interior of this glass tube, and of a diameter scarcely less than that of the tube itself, but not quite so long, is placed a thin hollow brass box, with its surface made as true as possible by turning in a lathe. This brass box is placed concentrically

with the outer tube, and is completely coated on its exterior surface with tin, the tin being acted on to the smallest extent by the ozone. This hollow box communicates with the exterior of the apparatus by means of tubes passing through the centre of the caps. It is intended that a current of water should circulate through this box. In the small annular space between the box and the glass tube oxygen is passed from the tubes fitted to the cap. The box itself forms one of the electrodes in connection with an induction coil, and a strip of tin-foil fixed to the outside of the glass tube forms the other.

Another interesting but different order of experiment, a discovery of M. Demoget, consists in replacing the plate of resin of an electrophorus by a membrane of caoutchouc. The membrane from a child's air-balloon strained on a metallic circle of 0·8 c.m. diameter, smartly rubbed with the back of the hand, the rubbed surface inverted upon a good conductor and the superior surface rubbed, will result in imparting to an insulated disc of 25 c.m. diameter sufficient electricity to discharge sparks of 3 to 5 c.m. length. The experiment may be utilised in the charging of an electrometer, or may be the source of amusement to the young.

The electrical theories advanced by M. Edlund, and that of electrolysis by M. Domalip, during the last quarter, would require more space for adequate consideration than can be afforded to a *resumé*.

M. Dupuy de Lôme describes the cryptograph of M. Pelegrin as an instrument designed to be raised on the ground, and to convert into expressions capable of being transmitted directly and secretly by telegraph, the polar co-ordinates of points which determine a given figure; whence the possibility with this instrument of following and interpreting, that is, of drawing, in Paris, *e.g.*, what correspondents in different parts of the country in telegraphic communication with Paris may see and telegraph but do not interpret.

TECHNOLOGY, &c.

In consequence of the illness of Dr. Joule, Dr. A. W. Williamson, F.R.S., presided over the Bradford Meeting of the British Association for the Advancement of Science. His Inaugural Address, which attracted the deepest attention, was delivered on the evening of the 19th of September. Owing to the meeting being held later in the season than usual we are prevented from giving an abstract of the proceedings in our present number. Evening lectures were delivered by Prof. W. C. Williamson, on "Coal and Coal-Plants;" by Dr. Siemens, F.R.S., on "Fuel;" and by Prof. Clerk Maxwell, F.R.S., on "Molecules." Dr. Russell, F.R.S., was President of the Chemical Section. He departed from the usual custom of reviewing the progress of chemistry during the year; and after a feeling allusion to the death of Liebig, and to his connection with the British Association, the attention of the audience was directed to the history of the colouring matter found in madder. Of the papers read before the Association, that which deservedly attracted the greatest attention was Dr. Ferrier's paper on the "Brain." We shall give an authentic account of these important researches in our next number.

The American Association for the Advancement of Science held its twenty-second annual meeting at Portland, Maine, beginning Wednesday, August 20th. The Officers of the meeting were—President, Prof. Joseph Lovering, of Cambridge, Mass.; Vice-President, A. H. Worthen; Permanent Secretary, F. W. Putnam; General Secretary, C. A. White. The attendance of members was large, and a goodly number of papers were presented. The following papers on chemical and closely related subjects were read:—"On the Silt Analysis of Soils and Clays," E. W. Hilgard; "Analysis of Mississippi Soils and Subsoils." E. W. Hilgard; "On the Distribution of Soil Ingredients in the Sediments obtained by Silt Analysis," R. H. Loughbridge; "On the Influence of Strength of Acid, and Time of Action on the Results of Chemical Soil Analysis," R. H. Loughbridge; "Remarks on Plate Lime-Glass and the Manufacture of Glass in General," L. Feuchtwanger; "The Chemistry of Copper Matte," T. Sterry Hunt; "Metaphysical Theory of Chemistry *versus*

the Atomic," Clinton Roosevelt. Subjects connected with geology and natural history formed the bulk of the papers.

At one of the "Public Conferences" of the French Association for the Advancement of the Sciences, which was also held in August, at Lyons, M. A. Gaudry delivered a lecture on the modern progress of chemical industry. He informed his audience that the amount of sulphuric acid manufactured annually in Europe amounts to 800,000,000 kilos., and would fill a canal 2 metres deep, 10 wide, and 25 to 30 kilometres in length. To yield this acid 800,000 tons of pyrites are yearly consumed. The condensation of the hydrochloric acid liberated in alkali works, the improvements of Mr. Weldon and Mr. Deacon in the manufacture of chlorine, the revolving soda-furnace, the extraction of potash as a secondary product in the manufacture of beet-root sugar, and the recent improvements in producing paper-pulp from wood, are among the principal points touched on in the remainder of this popular and able lecture.

M. Ruimet des Taillis, writing in the "Chronique de la Société d'Acclimatation," states that, by feeding silkworms on vine leaves, he has obtained cocoons of a magnificent red, and, by employing lettuce, others of a deep emerald-green. M. Delidon de Saint Gilles, of Vendée, has obtained silk of a beautiful yellow, other samples of a fine green, and others again of a violet, by feeding the silkworms on lettuce or on white nettle. He points out that the silkworms must be fed on mulberry leaves when young, and supplied with the vine, lettuce, or nettle leaves during the last twenty days of the larva-stage of their life.

For the preservation of gum arabic from mouldiness Hirschberg adds a little sulphuric acid to the solution, and finds that the mixture retains its adhesive property uninjured after the lapse of eighteen months.

M. Ducrot has published an interesting paper on apparatus for heating with hot air. From the theoretical point of view, the writer concludes that the quantity of calories furnished by the same apparatus, acting under the same conditions, is greater as the heated air issues at a lower temperature. By the same conditions, he means a constant external temperature, same quantity of fuel disposed in the same manner on the grating, burned in the same time with equal quantities of air. There is, however, for a piece heated with a given weight of fuel per hour, a maximum of temperature corresponding to a determinate quantity of air passing over the heating apparatus.

The following process was proposed by the late Professor Fuchs for fastening leather upon metal:—One part of crushed nutgalls is digested six hours with eight parts distilled water, and strained. Glue is macerated in its own weight of water for twenty-four hours, and then dissolved. The warm infusion of galls is spread upon the leather, the glue solution upon the roughened surface of the warm metal, the moist leather is pressed upon it and then dried, when it adheres so that it cannot be removed without tearing.

Steam has been proposed for extinguishing fires, by means of large pipes, communicating with a boiler, and capable of filling the building with steam in case of a conflagration.

In a paper on the spontaneous combustion of hay, H. Ranke says that, in consequence of prolonged fermentation, hay can become transformed into a true coal, which, when exposed to the air at somewhat elevated temperatures, acts as a pyrophorus.

M. Jobert has instituted researches on the history of digestion in birds: he finds that the gizzard is not exclusively a triturating organ, but a chemical stomach, which secretes an acid liquid.

According to E. Brescius beer may be clarified by means of tannin. For 1000 litres the author employs about 140 grms. of tannin, dissolved in 0·75 litre of water, which is thoroughly stirred up. After three or four days he adds 1 litre of isinglass or 2 of gelatine in the proportion of 1 kilo. to 100 litres. The complete clarification requires about eight days.

Struck with the inconveniences resulting from the use of toilet-soaps with a base of potash or soda, M. Bonnamy has prepared alumina soaps. These are, as a matter of course, neutral and free from causticity, and being insoluble in water their detergent action is simply mechanical, not chemical.

Ozokerit, which is now largely used in the manufacture of candles, is found in beds in the sandstone of Slanik, in Moldavia, in the neighbourhood of mines of coal and of rock-salt; it has also been discovered in the Carpathians. The material in its crude state is brown, greenish, or yellow; it is translucent at the angles, and its fracture is resinous. It is naturally brittle, but when softened can be kneaded like wax. It blackens on exposure to the air. It becomes negatively electric on friction, and exhales then the aromatic odour of a hydrocarbon. It melts at the low temperature of 66°. Its illuminating power is such that 754 ozokerit candles give a light equal to 891 of paraffin, or 1150 of wax.

The recent sudden destruction of two large passenger ships, the *Atlantic* and the *City of Washington*, has called attention to the desirability of availing ourselves of the means which modern science has placed at our command for the prevention of such disastrous accidents. For this purpose Mr. John Newlands proposes that each large passenger ship should carry a small but powerful steamboat or launch, and in foggy weather this steam launch should be sent on ahead some few hundred yards, being connected with the passenger ship by a flexible telegraphic cable provided with an electric battery, so that signals or messages might be continually transmitted from one to the other. The steam-launch should also carry an electric or other strong light, and be provided with a powerful steam whistle. On meeting with ice or with vessels, or unexpectedly approaching the coast, it would be comparatively easy to stop the steam-launch and give warning in time to save the passenger ship from danger.

General Morin gives a formula indicating what amount of air should be renewed hourly for each individual, in order that carbonic acid and vapours exhaled may not accumulate beyond a proportion of 0·0008 in a given enclosed space. He finds that in a cubic space of 10 cubic metres this renewal hourly should be 90 cubic metres; in 12, 88; in 16, 84; in 20, 80; in 30, 70; in 40, 60; in 50, 50; in 60, 40. Various applications of the formula are suggested—barracks, bedrooms, public halls, hospitals, &c.

Some improvements in photo-lithography have been effected by M. Paul. The paper is coated with a layer of white of egg beaten up and mixed with a concentrated solution of bichromate. When dry it leaves a hard smooth surface. After a sufficient insolation under the negative, the paper is covered with lithographic ink, then immersed in cold water to dissolve out the unchanged albumen, which is then removed with a fine sponge.

Horsky's diffusion apparatus does away with the rasping process in the manufacture of beet-root sugar, dispenses with three-fourths of the manual labour, and extracts the saccharine matter completely. The yield of sugar obtained by the use of this arrangement has this season amounted to 8·5 per cent, an amount greatly superior to that obtained in establishments where other processes for extraction are in use.

The following is the formula for Dr. Jeannel's horticultural manure:—

Nitrate of ammonia..	400 parts.
Biphosphate of ammonia	300 ,,
Nitrate of potash	250 ,,
Hydrochlorate of ammonia	50 ,,
Sulphate of lime	60 ,,
Sulphate of iron	40 ,,

At a general meeting of the Société Francaise de Photographie, held on August 1, 1873, a letter from M. Anthony, of New York, was read, offering the following prizes, open to photographers of all nations:—100 dols. for the best bust of a lady; 100 dols. for the best head of a boy under six years of age; 100 dols. for the best head of a girl under six years of age; 100 dols. for the

best group of two children under six years of age; 100 dols. for the best landscape. The proofs to be 16½ by 21½ centimetres, mounted on cards 25½ by 30½ centimetres.

At the same meeting M. Champion gave the following as the result of his experiments on the preparation of gun-cotton:—The acid mixture consists of 2 measures of nitric acid at 40° B., obtained by mixing common and fuming nitric acids, 3 measures of sulphuric acid at 66°. The mixture may be used either cold or at 40° C. The cotton is left in contact with the acid for three minutes, and the product washed till perfectly neutral.

Dr. E. Priwoznik records a change in cast-iron produced by the action of a mineral sulphur water. On examining an iron water-pipe which had been exposed for twelve years to the action of water rich in the sulphide of hydrogen, the innermost stratum was found to consist of—

Hydrated oxide of iron	81·08
Free sulphur	12·29
Sulphide of iron	4·48
Hygroscopic water	0·57
Nickel, cobalt, magnesia, silicic acid (soluble and insoluble), traces of carbon, and chlorides of ammonium and sodium	1·58
	100·00

This stratum is, therefore, an intimate mixture of hydrated oxide of iron, sulphide of iron, and sulphur. The hydrated oxide has the composition $2Fe_2O_3,3HO$, and is therefore identical with limonite. The middle stratum contained 79·2 per cent of metallic iron, and the exterior 92·6.

Interesting researches on the stroboscopic determination of the pitch of tones have been made by M. Mach. In the apparatus there is a cylinder which makes three revolutions in a second, and is divided into five octaves, At one end of it begins 10 bands, which, however, become more numerous and dense towards the other end, being there 320. To the axis of a syren is fixed a disc having equidistant radial slits of the same number as the holes in the syren-disc. The surface of the rotating cylinder is looked at through this slitted disc, while the syren tone is gradually raised. According to the stroboscopic principle the bands look distinct and at rest where there pass before the eye an equal number of them and of slits in the disc. If a scale of numbers of vibration be attached to the cylinder, the number of vibrations of the syren can be at once ascertained by observing the part corresponding to the distinct and still ring of the cylinder. One sees, however, distinct and at rest, not only the part of the cylinder corresponding to the number of vibrations of the syren, but also all those parts which correspond to the harmonic over tones. Of all such parts it is, of course, that one which furnishes the smallest number of vibrations that corresponds to the vibration-number of the syren. The determination may be varied in accuracy by varying the bands on the paper of the rotating cylinder. The apparatus may be applied to other sounding bodies. Thus let a mono-chord string be stretched at right angles to the axis of the cylinder; then simple teeth (Zachen) appear where the sounding string is opposite that part of the cylinder indicating the same number of vibrations. Another application is to attach small mirrors to tuning-forks, and watch in them the image of the rotating cylinder. An organ pipe may be also submitted to observation with aid of König's capsules and dancing jets.

ERRATA.—Page 474, footnote, for *plane* read *slane*. Page 480, line 22 from bottom, for *bog* read *boy*. Page 540, line 25 from bottom, for *north-western* read *south-western*.

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